HUMAN ABILITY

A CONTINUATION OF
"THE ABILITIES OF MAN"

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PREFACE

THIS volume was planned by Professor Spearman to be a continuation of *The Abilities of Man*. The greater part of it was written by him at Chesterfield during the war. It was my privilege to discuss each chapter with him, both by correspondence and during frequent visits to Chesterfield. But when he proposed that the book should appear under our joint names, I had to point out that my small share in it did not deserve such distinction. It was not till the 11th of June, 1942, that I consented, when I found that it was his unalterable wish.

I was looking forward to the pleasure of continuing this collaboration until the book was ready for publication. But it was not to be. I therefore crave the indulgence of readers for blemishes which may have in consequence crept in.

Special thanks are due to Mrs. Spearman for her encouragement when the book was being prepared for publication.

I am indebted to Professor Sir Frederic Bartlett, F.R.S., Editor of the *British Journal of Psychology*, for his permission to incorporate in this book the paper by Professor Spearman, entitled "Theory of General Factor", which appeared in the *British Journal of Psychology*, vol. xxxvi, part 3, May 1946.

It was my good fortune to enjoy the friendship of Professor Spearman for nearly thirty-two years. Many thrills I cannot forget: being invited to join his staff, to conduct his seminar, to take charge of the Psychological Research Laboratory of the R.A.F. in the First World War, to assist him when he carried out the first psychological testing in the Royal Navy; our many excursions together in Europe, Canada, the U.S.A., Africa, and

India; my pleasure at seeing him ranked as the world's premier psychologist. Is it then surprising that I have been profoundly influenced by Professor Spearman's psychological views and that I should do my utmost to promote them?

LL. W. J.

CAIRO
May 1948

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CHAPTER I

PRE-SCIENTIFIC NOTIONS

I. A New "Menace"
II. Monarchic Doctrine of Intelligence
III. Oligarchic Doctrine of Faculties and Types
IV. Anarchic Doctrine of Independent Elements
V. Help from Statistics

I. A NEW "MENACE"

AMONG the developments which at the present day are exciting the keenest interest, assuredly a high place must be assigned to mental tests. For the success of these can, with little exaggeration, be regarded as amazing, or even alarming. Over and above their attainment of extraordinarily wide acceptance, they threaten to exercise an immediate and revolutional influence upon daily life. In the sphere of education, for example, they claim to have won titanic victories over the whole system of examinations. Even more impressive, if possible, has been their invasion of military service. Already in the First World War they were applied to some 1,700,000 men of the American Army with such conspicuous success as to gain the warm official praise of the Secretary of State for War.

After being submitted to twenty more years of anxious consideration, they were introduced throughout the British military forces in the Second World War.

But there has still to be mentioned their most ambitious aim of all. Voices are becoming more and more insistent — for instance, in a recent parliamentary White Paper — that the sway of the tests must be extended over the whole national man-power, woman-power, and child-power. What *this* may ultimately mean, one scarcely dares to think! On the whole, then, the situation would appear to be promising, but not free from danger. Beyond all reasonable doubt, the tests do proffer such a potent aid to life that their renunciation would be suicidal. But on the other hand, the very fact of their potency should be a warning to handle them with circumspection.

An attempt by the senior author at their general presentation and evaluation was already adventured in 1926 in *The Abilities of Man*. But since that day — happily enough — great progress has been achieved. Accordingly, the following volume will chiefly stress the more recent advances. On many topics — especially those of a historical nature — reference must still be made to the fuller account given already in the older work.

II. MONARCHIC DOCTRINE OF INTELLIGENCE

In this way, the description of all pre-scientific study — let us say, before the advent of correlations — can receive from us little more notice than is afforded by the remainder of the present brief chapter. This will consist of a summary outline of the three fundamental doctrines which have led up to those which are now current and would appear almost indispensable for their right comprehension.

Earliest in the field came the ancient conception of a power that could be taken to provide the human mind with " universal " abstract ideas, and so to constitute the one source of genuine knowledge. Such a power was termed in Greek the " nous " ; accordingly, its actual exercise was " *noesis* ". Subsequently, these two words were rendered in Latin as *intellectus* and *intelligentia*. Still later on, these two words in their turn went over into English as respectively " intellect " and " intelligence ". But in the course of this last linguistic transition, there appears to have occurred a strange verbal freak ; the " intelligence ", from signifying the actual *exercise* of the intellectual power, was taken to mean *another power*. And thus a distinction that was intended to be merely grammatical was taken as being essential.

Furthermore, whereas the concept of the " intellect " has in the hands of philosophers been cherished more or less faithfully up to the present day, the meaning of "intelligence" has passed through many hands and undergone strange vicissitudes.

Its earlier and most unquestioning employment appears to have been very popular and — as one might have expected — very obscure. Far more rigorous was its subsequent usage in biology. Spencer,[1] for instance, defined it brilliantly enough as

> "the mental adjustment of internal relations to external relations."

All the more disappointing was its speedy lapse back again into extreme equivocality when called upon to explain and justify the modern device of mental tests. For in these the term intelligence was applied to a wholly indefinite hotch-pot of the most diverse things. Or, if any delimitations did ever gain authority, these flagrantly contradicted one another. For instance, some writers emphatically refused to admit among their intelligence tests any sort of memory, whilst others — or even the very same on other occasions — employ them more frequently than anything else. Almost as much may be said about intelligence tests including or not including imagination.

But if this be so and the notion of intelligence does embrace a wide, shifting, and even self-contradictory range, then all attempts to measure it would appear to become illusory. And indeed Binet himself — in contradiction to his actual practice — wrote plainly enough:

> "The mental faculties of each subject are independent and unequal; with a little memory there may be associated much judgment. . . . Our mental tests, always special in their scope, are each appropriate to the analysis of a single faculty."

III. OLIGARCHIC DOCTRINE OF FACULTIES AND TYPES

But herewith we arrive at the second fundamental doctrine of human ability. In distinction from the first called monarchic it may be termed oligarchic.

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[1] H. Spencer, *Principles of Psychology*, vol. i, pt. iv, chap. 2, 3rd Edn. (1881).

Here the single sovereign faculty is replaced by a small number of different ones. Almost synonymous have been such terms as "powers", "abilities", or "capacities". And essentially analogous have often been the usages of "types", and even "levels". Outstanding instances have been those of Judgment, Perception, Memory, Invention, Attention, Language, and Movement.

But all such faculties have had a strange fate.

Times without number they have been submitted to the most scathing criticism, and even ridicule. And yet they still appear to dominate the field, if not explicitly, at least implicitly. Scarcely anybody has ever tried to rebut the objections made to them. These are simply again and again forgotten. For Binet, as for countless others, Locke and Herbart might as well never have lived.

What then really were the objections so vehemently, but so vainly, brought against the advocates of faculties? By far the most fundamental was just the same as that which they themselves had made against the defenders of Intelligence; it consisted, that is to say, in the lack of evidence for functional unity. Indeed Herbart [1] had roundly asserted the contrary.

> "Strength of memory," he had said, "is usually limited in every man to particular kinds of objects. . . . He who easily remembers the technical expressions of a Science that interests him has often a bad memory for the novelties of town."

He winds up by complaining that:

> "The marvel of these things vanishes on discarding the hypothesis of mental faculties."

IV. ANARCHIC DOCTRINE OF INDEPENDENT ELEMENTS

After this fashion we have had to break up the few large faculties into a larger number of individually smaller constituents. But with this trend it is hard to see any stable halting place. Accordingly, there is little occasion for surprise on encountering a third doctrine where this is broken up into a multitude of independent elements. Such a view has found authoritative expression in the following passage of Thorndike:[1]

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[1] J. F. Herbart, *Lehrbuch z. Psychologie*, § 10 (1934). Cf. also the edition of 1816.

> "The mental sciences should at once rid themselves of the conception of the mind as a sort of machine, different parts of which sense, perceive, discriminate, imagine, remember, conceive, associate, reason about, desire, choose, form habits, attend to. . . . There are only the particular connections between particular mental events and others."

Despite such forcible exposition, however, this third or "anarchic," doctrine would seem to have never made much headway. For one thing, it is scarcely workable at all without some auxiliary hypotheses of averaging or of sampling which are hard to accept. But still much more serious is the fact that it suffers from just the same fundamental liability as the other two, only seen from the other side.

Both the monarchic and the oligarchic views broke down on the call to prove functional unity. But the anarchic view is in the little better case of having to prove functional independence, which is equivalent to disunity.

V. HELP FROM STATISTICS

In this chapter we have been sketching the views held about mental ability from the very beginning B.C. right up to some thirty years ago. Throughout there seem to have been three fundamental doctrines in mutual conflict; they are characterized respectively as monarchic, oligarchic, and anarchic. All three would appear to have failed at the same point; they have omitted to determine ranges of functional unity. By such a range is meant the sphere within which the ability at issue tends to vary concurrently from one person to another. Only when such a range is ascertained can there be an adequate basis for measurement, diagnosis, and prognosis; in short, for empirical science. But for any such going-together all this vast period offered neither proof nor disproof; any number of more or less striking instances would usually be intershot with, and seemingly contradicted by, a multitude of counter-instances; there remained only recourse to unverified assumptions.

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[1] *Educational Psychology*, iii, 366 (1914).

Now at last, however — with the genius of Galton — help arrived. It came in the form of a statistical device designated a "correlation coefficient". This is no longer baffled by contradictory cases, for it only measures the degree that the two values tend to correspond *on the whole*. When this correspondence is positive and perfect, the coefficient stands at +1·00. As the correspondence fades into randomness, the coefficient sinks to zero. And when, finally, the two values pass into an inverse relation, the coefficient assumes the minus sign up to the limit of −1·00.

Such coefficients have been constituted in considerable number and variety. But up to the present the most serviceable appears to have been that of "product moments" invented by Bravais.

CHAPTER II

THEORY OF TWO FACTORS

I. OBSERVATION OF CORRELATIONS
(1) Positive Signs. (2) Hierarchy. (3) Tetrad Equations. (4) Equivalent Criterion.

II. DEDUCTION OF "G" AND "S"
(1) Linear Factorization. (2) Non-linear Factorization. (3) Correlations of "G" and "S". (4) Measure of "G" and "S".

III. EXTENSION OF THEORY TO FURTHER FACTORS
(1) "Broad Factors". (2) Technique of Measurement. (3) Verdict of Further Observations. (4) Eclectic Doctrine.

IV. ERRORS OF SAMPLING
(1) Tetrad Differences. (2) Residuals.

V. UPSHOT

I. OBSERVATION OF CORRELATIONS

(1) Positive Signs. With this modern tool of correlation coefficient it was, then, that present-day psychology turned to the ancient problem of human abilities. But for some time further no great progress was won along this line. The more the correlations were accumulated and arrayed, the more obscure became the picture.

Eventually, however, a light did dawn on the scene. It came in the guise of what was designated the theory of "Two Factors". To this, accordingly, we will now betake ourselves. For it would appear still to hold the key to the situation.

The earliest fundamental observation made was that the inter-test correlations, although widely varying in magnitude, were at least regularly positive in sign. On behalf of the old and charitable view, that a person's inferiority in one kind of performance is likely to be compensated by superiority in another, there was found no support whatever. On the contrary, it appeared that any failure at anything is rather a bad than a good augury for all other things.

(2) Hierarchy. But soon the correlations manifested another general tendency; one which, although far less universal, proved to be even more pregnant with scientific significance. It was to the effect that, under certain conditions, the correlations were such as to allow of being drawn up in what was called a "hierarchy". Below is an illustration (imaginary).

	1	2	3	4	5
1	..	·80	·60	·30	·30
2	·80	..	·48	·24	·24
3	·60	·48	..	·18	·18
4	·30	·24	·18	..	·09
5	·30	·24	·18	·09	..

(A)

Here the five headings indicate as many tests. whilst the decimals are their correlations. As will readily be noticed, the latter are hierarchical in the sense of continually becoming smaller (or equal) from left to right and also from top to bottom.

(3) Tetrad Equations. A more definite expression of this hierarchical tendency has been obtained in various ways. All should lead, at any rate approximately, to the same result. But supremely characteristic among them has been the breaking up of the whole set of correlations into elementary sets of four tests each. Such a set has been called a "tetrad". In this fashion, the hierarchy at once yields a system of equations having the following form of equal ratios:

$$\frac{r_{ac}}{r_{ad}}=\frac{r_{bc}}{r_{bd}}. \qquad \text{(B)}$$

For illustration, let here a, b, c, and d be taken to consist respectively of the tests 1, 2, 4, and 5 in the hierarchy (A). Accordingly, the equation (B) becomes

$$\frac{r_{14}}{r_{15}}=\frac{r_{24}}{r_{25}}.$$

Thence, picking out the actual values from (A), we get

$$\frac{\cdot 30}{\cdot 30}=\frac{\cdot 24}{\cdot 24},$$

which is obviously correct.

But the preceding equation, simple as it is, admits nevertheless of several conversions, each of which supplies a master key to a whole field of statistical operations. One that is greatly employed, especially in connection with sampling errors, is the following: it may be called the rule of zero tetrads.

$$r_{ac}\cdot r_{bd} - r_{bc}\cdot r_{ad} = 0. \tag{C}$$

Yet another most important conversion is:

$$r_{xx'} = \frac{r_{xa}\cdot r_{x'b}}{r_{ab}}, \tag{D}$$

where x and x' are any two tests whose fit into the hierarchy lies under suspicion.

(4) Equivalent Criterion. Although the theory of Two Factors is so essentially based on the equations (B) and (D), still it may be of some interest to point out an equivalent criterion to the theory of Two Factors. It has been obtained along the lines of the well-known operation of correcting correlations for "attenuation". For any two test-scores x' and x'' may be regarded as fallible measures for their common constituent x. Similarly, y' and y'' for y. Thereupon to satisfy the hypothesis, that all the correlations between x', x'', y', and y'' derive from a single common factor, requires only that the corrected value r_{xy} should be equal to unity. And this has actually been observed.

II. DEDUCTION OF "G" AND "S"

(1) Linear Factorization. In this manner we are led from the first half of the theory, which centres on the observation of (B) and (D), to the second half of it, which thence proceeds mathematically to deduce factors. This was done by proving that when such observations as (B) can be made, then, and only then, every one of the test-series at issue can be written in the following form:

$$M_{ax} = r_{ag}\cdot G_x + r_{as_a}\cdot S_{ax}, \tag{E}$$

where M_{ax} is the total score of any individual x at any test a; G_x is a part-score or "factor" common to all his scores at the different tests respectively; whilst S_{ax} is the remainder of his score at test *a* and constitutes a factor specific to it in the sense of being uncorrelated, either with the common factor, or with any other of the specific factors. In this equation (E), too, lies the reason for the sometimes challenged right of the theory to be called that of "Two Factors". This title only indicates the initial degree of analysis; certainly not the ultimate.

(2) Non-linear Factorization. It should be noticed, however, that G and S_a are simply added together; in other words, M is only a linear function of them. But this simple form of function, although almost universally adopted (on account of its great convenience), is in point of fact an extremely improbable one. Accordingly, there is considerable interest in determining how the matter would stand in the case of any non-linear function. Thus the combination of G and S_a instead of being additive might be taken as multiplicative; and then (E) might be replaced by

$$M_{ax} = p_a . G_x \times q_a . S_{ax}. \tag{F}$$

But, unexpectedly, the conditions required for the possibility of expression in this non-linear form (F) turned out to be the same as those for (E).

(3) Correlations of "G" and "S". In both cases alike, then, the analysis will be accomplished when the *required* correlations of the scores with the factors have been expressed in terms of their *known* correlations with one another. And this expression has been shown to be achieved as follows:[1]

$$r^2_{ag} = \frac{r_{ab} . r_{ac}}{r_{bc}}, \tag{H}$$

whence easily

$$r^2_{as_a} = 1 - \frac{r_{ab} \cdot r_{ac}}{r_{bc}}, \tag{I}$$

and

$$r_{ag} . r_{bg} = r_{ab}. \tag{J}$$

If the triplet (B), (C), (D) may be regarded as the foundation of the theory of Two Factors, then surely (H), (I), (J) constitute its spearhead. A vital feature of them is the evident fact that at the limit they only need a single pair of variables, namely, *b* and

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[1] C. Spearman, *The Abilities of Man*, Appendix, p. xvi.

c: these two operate as reference values in respect of *a*. But instead we can, if we will, go to the opposite extreme and employ for reference every pair of variables that may happen to be included in the set at issue and to be subject to (B). Usually, however, the wisest course is a compromise; this consists in picking out all the pairs that appear to be reasonably free from legitimate objections, and then to average out all the results from these only. They should already be in tolerable agreement with one another. The averaging should only serve to eliminate the random errors.

(4) Measure of " G " and " S ". The foregoing analysis of the observed test-scores in terms of the hypothetical factors G and S demanded as its natural sequel the measurement of these factors themselves. Now to obtain an estimate of G_x the simplest way would be to utilize (E) and employ the regression equation

$$G_x = r_{ag} . M_{ax}. \qquad \text{(K)}$$

But, since here, usually, there are many such equations available, a procedure was devised for combining all the test-scores into a single best-weighted average [1] and then use the regression equation. To obtain an estimate of the specific factor, G_x was put into (E), thus yielding

$$S_{ax} = \frac{M_{ax} - r_{ag} . G_x}{r_{as_a}}. \qquad \text{(L)}$$

III. EXTENSION OF THEORY TO FURTHER FACTORS

(1) " Broad " Factors. So far, our account of the theory of Two Factors has been limited to the case where one of the two is present in all the tests, whereas the other is confined to a single one and, therefore, is in this sense specific. The formulae (B) to (J) have indeed been taken as quite general in that *a*, *b*, *c*, *d* have stood for any or all of the tetrads of tests concerned. But still within each tetrad the specificity has been unbroken.

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[1] *Ibid.* Appendix, pp. xviii ff.

What, then, has been the attitude of the proponents of the theory towards the so-called "group" factors; that is to say, those which may occur in more than one of the tests but yet not in all? The question is important, because it has led to much misunderstanding. Now the view held in the theory has been that to a large degree group factors are only relative to the composition of the set of tests at issue. *Any* test can at will be made to include any number of them, it was said, simply by introducing into it other tests sufficiently akin.

Nevertheless, from the beginning, the possibility was suggested that some of the factors might cover such a broad range as to render the occurrence of the said grouping, at any rate, frequent; not indeed as an alternative to the general and specific factors, but, so to speak, superposed upon them.

To the investigation of these "broad" factors it was, that further research principally devoted itself.

(2) Technique of Measurement. Now for this quest after broad factors, just as in the search for G and S, the primary tool was once more supplied by way of the correlations. And a great step was already achieved in this direction by all our equations (B) to (K); but most explicitly by (D). For in every case where any of these is not adequately satisfied by the general factor, is indicated the presence of some or other group factor.

Far more significant, however, was the next step; for by its means the advance was made from the aforesaid bare indication of the presence of the group factors to their definite measurement. For this purpose use was made of partial correlation. This yielded the following equation (used almost throughout the research chronicled in *The Abilities of Man*):

$$r_{s_a s_b} = \frac{r_{ab} - r_{ag} \cdot r_{bg}}{(1 - r^2_{ag})^{\frac{1}{2}} \, (1 - r^2_{bg})^{\frac{1}{2}}}, \qquad \text{(M)}$$

where $r_{s_a s_b}$ denotes the required group correlation between the tests a and b. It is additional to the correlation between them due to G, and so represents some additional factor. On due standardization of the scores, it may be remarked that the denominator in (M) simplifies to unity. On its so doing, the equation becomes that which ever since has been almost universally employed to measure "residuals":

$$r_{s_a s_b} = r_{ab} - r_{ag} \cdot r_{bg}. \qquad \text{(N)}$$

These then are arrayed in a new table corresponding to that of the original correlations.

This new table is then submitted to exactly the same treatment as the original one. In particular, use is again made of the equations (B) to (J).

There may, or may not, ensue a further residual and therefore a third factor. And so on, until the residuals do disappear.

(3) Verdict of Further Observations. Such was the technique devised for the purpose of detecting and measuring "broad" factors. Equations (B) to (J) were employed from the very beginning. Even (M) appeared at any rate in *The Abilities of Man*, 1927. The first positive discovery by such means was already in 1906, when an overlap was observed between the memorization of syllables and that of numbers.[1] From that day onward such broad factors capable of embracing very extensive ranges of ability were placed in the forefront of inquiry.

The eventual verdict was that the then still prevalent faculties and job analyses had but little support in fact. On the other hand, there were said to be

> "exceptional cases where on the contrary specific correlations do become of appreciable magnitude."

Specially mentioned among these were the verbal, logical, mechanical, the psychological, and the arithmetical abilities. All these, it may be remarked, were in the following year found by Kelley[2] to be in quite remarkable agreement with his own results.

(4) Eclectic Doctrine. The foregoing results were said to indicate that, after all, there was some modicum of truth in each of the three great and ancient views set forth in the preceding chapter. For "the monarchic doctrine of intelligence" could claim to be, at any rate, the forerunner of the modern G. Again, the "anarchic doctrine of independent elements" forestalled in some degree the present conception of the multitudinous narrow S's. And as much was said, finally, even about the faculties of the "oligarchic view". For something like these — albeit shrunken indeed — was taken

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[1] Krueger and Spearman, *Zeit. f. Psychol.*, xliv, 102 (1906).

[2] T. L. Kelley, *Crossroads in the Mind of Man*, p. 22 (1928).

> "to have revealed itself in what we have been calling the broad factors."

These were described as

> "quite distinct from the universal factor and fairly distinct from the ordinary narrow factors." [1]

IV. ERRORS OF SAMPLING

(1) Tetrad Differences. There remains one more clause that for the theory of Two Factors is fundamental. In common with all other practical statistics, it deals, not with the entire population ultimately at issue, but only with samples of it. These here, as elsewhere, inevitably introduce errors which fatally block further progress, until at least we can form some estimate of their probable magnitude.

At this point more than any other it was that the theory of Two Factors met with a definite check lasting many years. We wanted to find the sampling "probable error" of (B), but did not know how to do so. At last a tolerable substitute was discovered; it was the probable error of what had been called the intercolumnar correlations and had at times been used in place of (B).

Quite recently, it may be mentioned, there has been some interesting returns, both to the intercolumnar correlation, and to the probable error of (B). At the same time, however, relief from the check was found by switching over from (B) to (C). The probable error of the latter equation was published in 1924.[2]

(2) Residuals. Only second in importance to the probable error of the tetrad difference (C) is that of the residual (M). But the latter consists essentially of a partial correlation, so that its error is more or less approximately known.

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[1] *The Abilities of Man*, p. 84.

[2] C. Spearman and K. Holzinger, *Brit. J. Psychol.*, vol. xv (1924).

V. UPSHOT

This chapter has been principally descriptive of the original statistic method used in the theory of Two Factors. This has been shown to involve three main steps: (1) the finding (or devising) of any system of tests that satisfy (B); (2) the application of the theorem (H); (3) the application of (M) or (N). In Chapter IV we shall see how the method of factorizing has subsequently been developed further. As regards the statistical results of this method, these have only been outlined. They have consisted of (1) a general factor; (2) an unlimited number of narrow specific factors; and (3) very few broad group factors. All these will be resumed and supplemented, and also the *interpretation* of these and other results will be discussed in later chapters.

CHAPTER III

SOME CRITICISMS

I. Group Factors
    (1) Theoretical Misunderstanding. (2) Historical Claim.
II. Uniqueness of "G"
    (1) Identification. (2) Transformation. (3) Determinateness.
III. Philosophical Doubt
    (1) Multiple Divisibilities. (2) Real Existence. (3) Hypotheses.
IV. Statistical Policy
    (1) Range of Significance. (2) Embarrassment with False Trails.
    (3) Negative Proof.
V. Upshot

I. GROUP FACTORS

(1) Theoretical Misunderstanding. After the foregoing account of the original theory of Two Factors, let us proceed to consider its subsequent developments. These fall conveniently into two portions, which are respectively destructive and constructive. Only the former constitutes the theme of the present chapter ; we want to see what opposition the theory has encountered. Certainly the criticisms have been extensive and emphatic. All the more to be regretted is that they must here be treated with great brevity. Fortunately, however, a much more detailed account of them has been supplied elsewhere.[1] Now of all the objections raised by the opponents of the theory, none has been so frequent and pressing, and yet none can be so summarily settled, as the charge that it conflicts with the discovery of group factors. For in reply the proponents of the theory could at once point out that the theory did not require that group factors should be absent altogether — which would be absurd — but only that they should be so over the very limited range required as a jump-off (see preceding chapter). To find (or, if need be, invent) such a limited range is an achievement whose feasibility seems never even to have been challenged.

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[1] C. Spearman, "Factor Theory and its Troubles", *J. Educ. Psychol.*, vols. xxiii and xxiv (1932–33).

(2) Historical Claim. Later on, however, the opponents of the theory appear to have changed their ground. They began to urge that its advocates had, at any rate, only been converted to the group factors recently.

But in this fashion the point at issue, from being fundamental, came down to being merely historical. And on this footing the advocates raised a counter-claim. They themselves, they protested, had been the first to discover nearly all of the group factors measured ever since. Moreover, they said they had begun these discoveries more than forty years ago; that is to say, long before their opponents had entered into the field at all.

In view of this discrepancy between the two different accounts of the matter, reference for the actual facts must be made again to Chapter II. However, the argument is apt to become tricky. Thus even such an able and sympathetic writer as Burt [1] quoted the senior author as holding that the group factors had — up to that date — been found small *and* rare. Really, he had expressed the widely different view that such factors have been small *or* rare.

II. UNIQUENESS OF "G"

(1) Identification. Of quite other gravity than the preceding question of the occurrence of group factors has been the problem of the unity of the G-factor. The former trouble, as we saw, was either a mere misconception of the theory at issue or else only a historical detail. But the later difficulty was genuinely menacing for the entire theory. Put into the form of a criticism, it found such expressions as the following:

> "When a G is found, it may not be the G which another found yesterday."

So, too, still more drastically,

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[1] C. Burt, *The Factors of the Mind*, pp. 143 and 297 (1940).

> "The individual has as many G's as you administer tests."

And indeed the concession must be made that all such identifying does require definite supporting evidence. On the other hand, however, such evidence is said to have been duly supplied. In its most exact form the identification runs as follows:

> Let there be two hierarchical sets of tests having as their respective general factors G_1 and G_2. Then since the first set is by assumption hierarchical, all that is common to any two or more of the tests can only be G_1. But again, if these same two or more tests belong also to the second set, then all that is common to them can only be G_2. Hence G_1 and G_2 must be one and the same.

Less systematic, but scarcely less cogent, have been the general agreements found between different determinations throughout factorial literature. But of this we shall see much more later on.

(2) Transformation. Kindred has been the following objection. Suppose for argument's sake that some or other set of tests has been found hierarchical, so that each of the tests in the set does admit of having all the scores analysed into terms of a general and a purely specific factor. From these primary variables — as we may call them — there can evidently be derived an immense number of secondary sets made up of the primary scores variously weighted. And then this weighting has been shown to be possible in such wise that the secondary scores also are hierarchical, but, nevertheless, this time the G is different; it has been "transformed". From this fact the deduction is made that after all the G is merely "relative to the set-up".

But here once more there has been found a counter-argument. Admitting the evidence that such a transformation is conceivable, further mathematical inquiry has shown it to be only a freak case. Not only would it involve psychologically ungrounded negative weightings, but these would throughout have to be just large enough to cancel out the concomitant positive ones. For such a complete system of coincidence to occur by chance would be a miracle.

(3) Determinateness. More subtle has been the reproach urged against G of being in part indeterminate. In symbols, so it is said,

$$G = r + i,$$

where only r is determinate, not i. But this contention has been disarmed by the proof that this intruding " i ", if not absolutely removable, can, at any rate, be made indefinitely small, which is practically just as good.

A more fundamental defence against the objection has been the distinguishing between indeterminateness of two different kinds. On the one hand, there is what may be regarded as a mere lack of exactitude; this of course affects in greater or less degree all measurements whatever, but does not influence the object to be measured. For example, the distance between two points may be measured several times with results more or less unequal every time; but still there may be no reason to postulate any change in the distance itself. The other kind of indeterminateness is more basic and does affect the object to be measured. As an example, take the measurement of a person's head; here variation may occur, not only in making the actual measurement, but even in conceiving what is "length". Now the indeterminateness of G with which we are here concerned has been shown to be of the former of the two preceding kinds; that is to say, it is only a defect of exactness. In fact, it is at bottom nothing more than the ordinary error of a regression equation.

It admits of wide variations (including those derived from the nature of the populations in which the individual happens to have been included). But the "true" G it leaves intact.

III. PHILOSOPHICAL DOUBT

(1) Multiple Divisibilities. Next, let us briefly record some criticisms that have taken a still loftier flight. A conspicuous instance has been the sweeping assertion that the test-scores

> "must not necessarily be divided according to the two-factor theory, *for* other modes of division are possible."

Here we appear to have a tacit assumption that different modes of dividing are in mutual conflict.

Now something of the sort no doubt does hold good in certain concrete physical cases. Thus we cannot fashion a tree into a mast and at the same time cut it up into matches. But there are other cases, even physical, where this is no longer true. For example, any ability can quite well be conceived as dependent on the single factor of energy and yet *at the same time* on innumerable neurone factors. As for the case of statistical or mental factors, these have been shown to possess a surprising variability and pliability.

(2) Real Existence. Still more frankly metaphysical has been the reiterated charge that

> "the entities represented by G and S may not actually exist."

So, too, another author has written that

> "the vanishing of the tetrads constitutes in no sense proof of the *existence* of G and S."

But here the unwary critic might be tempted to fly off at a tangent into Cartesian doubt as to whether he really exists himself! And no such argumentative escapade seems very helpful for the progress of science. Possibly, indeed, the present volume may upon occasion be led into dangerous proximity to metaphysics; notably in reporting the attempts that have been made to provide G with an explanation.

So long, however, as we remain within the sphere of pure statistics, the matter appears to be simple enough. We will accept the ordinary usage of words and will take (B) and (K) to have been proved to exist really when, and only when, the available evidence has shown these equations to be actually and stably satisfied. The case of G and S is quite analogous. These values, too, will be taken to exist really whenever they have been actually observed or otherwise proved.

(3) Hypotheses. At this point, mention may be made of yet another of the more or less philosophical criticisms that have enjoyed considerable support. In a way it contrasts with the one just cited. For whereas that demurred at claims to deal with real existence, the present objection is, instead, to dealing with hypothetical existence.

The supreme instance has already been considered in this chapter. It is the case of G being interpreted as deriving from some sort of psycho-physiological " energy ". In defence of such an unproved and perhaps unprovable hypothesis, the plea must be entered that physical science lies in precisely the same predicament. Its principal concepts (force, gravity, temperature, refraction, pressure, density, electricity, magnetism, and so forth) are all essentially hypothetical. And the same excuse that serves physics must do also for psychology. This is only pragmatic. It consists in the hope that the hypotheses may be introduced in such fashion as to aid the progress of empirical science. How far this hope has at present been realized cannot well be judged until, at any rate, after marshalling all the chief relevant facts.

IV. STATISTICAL POLICY[1]

(1) Range of Significance. To close this chapter, let us chronicle some rather serious differences of opinion in respect of what may be called general statistical policy.

Such a conflict has been about the range within which any experimental observation ought not yet to be accepted as " significant ". Now on this matter Udny Yule had authoritatively written :[2]

> " We cannot feel any great confidence that it (the datum obtained by sampling) is likely to be significant unless it exceeds *four times* the probable error."

But by some recent authors this ratio of four to one would seem to have been thrown overboard.

Thus Garrett and Anastasi[3] have so treated a well-known correlation amounting to ·20 with a probable error of ·13. For here the critical ratio is only about three to two. Nevertheless, the authors protest that to deny significance here is " almost certainly wrong". Their line of argument is that even a ratio no higher than 3 : 2 will by mere chance occur only about "once in six times". This argument, however, has been shown to be misleading. For the alleged ratio of once in six times applies really to two areas of probability which *both* consist of insignificant cases. Whereas the comparison properly required is between cases that are not significant and those which are so.

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[1] C. Spearman, *J. Educ. Psychol.*, xxiii, 481 (1932).

[2] G. Udny Yule, *Theory of Statistics*, 6th Edn., p. 311 (1922).

[3] *Annals of the New York Academy of Sciences*, vol. xxxiii (1932).

On the whole, then, Yule's ratio — or something similar — still holds the field.

(2) Embarrassment with False Trails. However, a more practical counter-argument would appear to have been supplied by Garrett and Anastasi themselves. They constructed random samples of 25, 50, 100, and 200 individuals respectively, in all cases alike there was really no group factor at all. Nevertheless, more than a quarter of the observed tetrad differences exceeded the said ratio of ·20 to ·13; accordingly, all these tetrad differences would have been falsely accepted as probably significant.

And by starting all these false trails, the whole situation becomes encumbered, confused, and embarrassing.

It wonderfully clears up if, in all cases of a ratio of less than four times the probable error, the investigator resolutely suspends his judgment, and betakes himself to improving his evidence.

(3) Negative Proof. One more kindred point is that raised by Thomson,[1] when he urges that a special test is needful to prove that any observation is *not* due merely to the error of sampling. Indeed he goes so far as to say that the lack of any such a specially disproving test is

> "one of the most serious blemishes in factorial analysis."

But here we cannot agree. We would rather suggest that strictly speaking the demanded disproof is a sheer impossibility. The investigator is impotent to demonstrate that any item, mental or even physical, is absent; he can at best only show it to be of inappreciable magnitude in comparison with the error of the experiment.

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[1] *The Factorial Analysis of Human Ability*, p. 156 (1939).

But so we come round to the same point as before. We seem obliged to place the burden of proof on existence, not on non-existence. The latter only wins by default.

V. UPSHOT

This concludes our brief account of what has been done recently (especially after 1927) by factorial psychology on its purely critical and destructive side. On the whole, there seems to have been much cry but little wool. The great bulk of objections brought forward have been only based on misunderstandings.

To a much smaller extent, however, the criticisms have been reasonable enough. But even then, they have led, not so much to any correction of errors, as rather to a deepening of foundations. Anyway, as already mentioned, the whole subject has been treated much more fully elsewhere.[1]

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[1] *J. Educ. Psychol.*, vols. xxiii and xxiv (1932–33).

CHAPTER IV

DEVELOPMENT OF METHODS

I. Elaboration of Principles
  (1) Philosophical Rigour. (2) Spatial Representation. (3) Matrix Notation.

II. Direct Methods
  (1) " Bi-factors ". (2) Simple Summation and Weighted Summation. (3) Centroids. (4) Principal Components. (5) Agreement of Values.

III. Indirect Methods
  (1) Expression in Series. (2) Negative Factors. (3) Rotation. (4) Oblique Axes.

IV. Further Methods
  (1) Partial Correlation. (2) Variance. (3) Inverted Factors.

V. Various
  (1) Appraisal of Persons. (2) Errors of Sampling.

VI. Summary

I. ELABORATION OF PRINCIPLES

(1) Philosophical Rigour. The preceding chapter aimed at recording the chief attempts which have been recently made to submit the new theory of " Factors " — and particularly that of " Two Factors " — to destructive criticism.

Let us now turn to consider what has been done about this theory on its more constructive side.

The achievement may conveniently be divided into two main sections, namely, the development of method and the accumulation of observations. But for us here the latter of these will be paramount. And, accordingly, the fulfilment of the other purpose, that of method, must remain highly condensed. What could be (and indeed has been) said about it might well fill several volumes ; but to it here can be spared no more than this single chapter.

On account of this restriction we must refrain from the temptation to join in the fray which is now endeavouring to establish the factorial theory on a more rigorous philosophical basis. Not for us, then, is the luxury of exploring the depths of ontology and epistemology. We are under no obligation to answer such questions as: Whether "factors" have any "real existence"? do they admit of genuine "measurement"? does the notion of "ability" involve at bottom any kind of cause, or power? Or is it only intended for the purposes of bare description? How, if at all, does the role of factors transcend that of classes? And much more.

At their time and in their place such themes are doubtless well enough. The senior writer himself has indulged in them not a little. *Dulce est desipere in loco.* But for the present purposes he has felt himself constrained to keep within the limits of barest empirical science. These he takes to be at bottom nothing but description and prediction. From a slightly different standpoint, it is still reducible to predication of existence, coexistence, and sequence. The rest is mostly illumination by way of metaphor and similes.[1]

(2) Spatial Representation. On a very different footing from any such delving of the factorial theory into the profundities of metaphysics has been its ever-increasing promotion in the realm of mathematics. Sometimes, indeed, this tendency has been almost pushed to excess; there has been an intolerable deal of mathematical sack with but a pennyworth of psychological bread.

In the present volume, however, we will take especial pains to steer wide of any such needless difficulties. Eventually, no doubt, the serious student of factorial psychology will have to grapple with much that he was not obliged to learn at school. But usually the gist of the matter — as this will be here set forth — should be intelligible to him forthwith. Turning, then, to the mathematical progress which even in the present short account cannot altogether escape notice, a large part has been concerned with the device of representing test-scores and their correlations by means of their analogies in the characteristics of space. Thus in the work of Garnett,[1] followed by that of Thurstone [2] and Holzinger,[3] the scores were usually regarded as so many vectors with a common origin; the correlations between the scores were shown to be equivalent to the products of the lengths of the said vectors by the cosines of their angular separations. Thus in the following simple illustration any two scores may be represented by the lines OA and OB, whilst their correlation is measured by the cosine of the angle θ. OE and OD are the projections (and therefore measurements) of OA and OB respectively on the horizontal axis. Similarly, OG and OF in regard to the vertical one.

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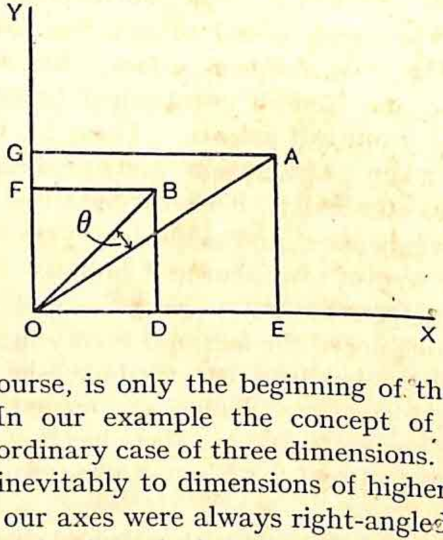
[1] C. Spearman, *Psychology Down the Ages*, ii, 17 (1937).



And this, of course, is only the beginning of the potentialities introduced. In our example the concept of "space" was limited to the ordinary case of three dimensions. But the study of factors led inevitably to dimensions of higher order without limit. Again, our axes were always right-angled, whereas need has been found for introducing also the oblique kind. With these and other complications, there are brought on the stage an endless swarm of further concepts, formulae, equations, and theorems about "common factor space", "subspace", "hyperplanes", and so forth.

(3) **Matrix Notation.** What has just been said about spatial representation applies also in large degree to another potent mathematical device; that of algebraic matrices. A matrix is,

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[1] Garnett, *Brit. J. Psychol.*, vol. ix (1919).
[2] Thurstone, *Vectors of the Mind* (1935).
[3] Holzinger and Harman, *Factor Analysis* (1941).

of course, merely a system of numbers arranged in a rectangular array of m rows and n columns. For instance:

$$\left|\begin{array}{ccccc} a_{11} & a_{12} & \cdot & \cdot & a_{1n} \\ a_{21} & a_{22} & \cdot & \cdot & a_{2n} \\ \cdot & \cdot & \cdot & \cdot & \cdot \\ \cdot & \cdot & \cdot & \cdot & \cdot \\ a_{m1} & a_{m2} & \cdot & \cdot & a_{mn} \end{array}\right|$$

Among the principal elements for which such a's have been made to stand are the coefficients of the common factors. When such matrices have been duly arrayed, they are submitted to various operations, such as those designated " transposition " and multiplication. A much used instance of the latter kind is the following:

$$S = F \times P,$$

where S stands for the matrix whose elements are the test-scores of the individuals; in P they are their factor-scores; and in F they are the correlations of the tests with the factors. The cross indicates the matrix multiplication.

All such acute and comprehensive elaboration of theoretical form — both spatial and matrical — has naturally enough been full of promises of a great advance in factorial theory and practice.

II. DIRECT METHODS

(1) " Bi-factors ". With or without utilizing the aforesaid extended mathematical equipment, however, the study of the factors soon showed that the crucial relation (H) described in Chapter II admits of further developments. In the first place, the discovery was very early made that innumerable other forms of analysis were no less possible; these at least stood in need of exploration. Moreover, as has already been shown above, there exist certain conditions where the Two Factor analysis ceases to be applicable at all. On both accounts, then, there was need of something new.

Now, the development most directly advancing out of the original Two Factor view was that which has been designated by its authors — Holzinger and Swineford[1] — as "bi-factor". The following is their description and claim:

> "The form of the bi-factor was conceived as a direct extension of Spearman's 'Two-factor' solution in the case of sets of variables which do not meet the assumptions underlying the latter solution. For a specialized set of variables the 'Two-factor' pattern, *i.e.* one general factor, may be sufficient to reproduce the correlations. With a complex set of variables, however, the criteria for a single general factor may not be satisfied, and group factors may be required. The bi-factor solution makes allowance for both general and group factor."[2]

But as regards this comparison of the two methods some comments may be illuminating. In particular, the two, so far as they respectively extend, yield exactly the same results. This may be immediately seen by comparing (H) with the formulation of Holzinger.[3]

Where, however, the bi-factor method must be conceded to have really surpassed its Two Factor predecessor, consists in its introduction of the "belonging coefficients" or B-coefficients.[4] These have the supreme merit of linking up at last the statistical with the psychological data. As claimed by Holzinger, they are

> "based on the assumption that the variables of a group identifying a factor should have higher intercorrelations than with other variables in the set."[5]

(2) Simple Summation and Weighted Summation. Besides the foregoing bi-factor, and chronologically even earlier, came another outstanding attempt at extending the scope of the factorizing. This was contributed by Cyril Burt.[6] Whereas the older method had dealt with every triad (H) separately, the newer way took the sum or the mean of each whole column of correlations together. Using the customary symbols, the critical value changed from many repetitions of the triad $r_{hx} . r_{hy}/r_{xy}$ to the one single case of M(r_{hx}). Subsequently Burt[1] has considerably developed a Simple Summation Method and a Weighted Summation Method for single-factor analysis and also for multiple-factor analysis.

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[1] *The Stability of Bi-Factor Solution* (1939).

[2] *Factor Analysis*, p. 101 (1941).

[3] *Ibid.* p. 117, Formula (6.13). [4] *Ibid.* p. 116. [5] *Ibid.* p. 24.

[6] *The Distribution and Relations of Educational Abilities*, p. 53 (1917).

(3) Centroids. A very widely adopted method of factorizing is the "centroid" method due to Thurstone. His fundamental equation is[2]

$$a_{\kappa 1} = \frac{r_\kappa}{\sqrt{r_t}}. \qquad (0)$$

Here r_κ is the sum of all the correlations in the correlation column κ; r_t is the sum of all the correlations in the whole table of them; whilst $a_{\kappa 1}$ is the required correlation of the test κ with the first factor. It will be noted that this is Burt's formula, but Thurstone has diverged in his treatment of the diagonal cells.

In order to obtain this result, however, the data have needed several not unimportant auxiliary steps. Least troublesome, perhaps, is the assumption that a performance can be described as a sum of weighted linear contributions of independent factors.

More questionable has been, however, the step whereby the vacant diagonal cells in the table of correlations are filled out, so as to make the whole table into a solid rectangular block. Commonly such stop-gaps have been supplied by the so-called "communalities". These are defined as

> "the sum of variance due to abilities which are common to two or more of the tests."[3]

But actually these fillings have altered more or less arbitrarily from one occasion to another; indeed at their limit they have varied from unity down to zero. Thurstone himself has proposed many different values and seems to have ended by preferring whatever may happen to be the highest in the whole column at issue. But Holzinger, on the other hand, finds the most satisfactory value to be afforded by the columnar average (see his "averoid" method [1]). But in all this matter there may still be missed any explicit justification for introducing such a stop-gap at all. Certainly it much increases mathematical tractability. But is it equally favourable for probability?

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[1] *The Factors of the Mind*, Appendix I (1940).

[2] *The Vectors of Mind*, p. 94 (1935). *Multiple-Factor Analysis*, p. 152 (1947).

[3] *The Vectors of the Mind*, p. 62.

Far greater opposition, however, has been roused by another feature of this "centroid" method: this is the characteristic which has supplied its name. It consists in that the first reference axis is quite arbitrarily made to pass through the centroid of the factors for all the tests. Hereby all the signs become independent of the experimental data and are instead wholly predetermined by the method employed. Moreover, the result in itself would appear to be irrational. For the signs of every factor (except the first) become exactly as much negative as positive. That is to say, it is in precisely half the tests not an ability at all, but a disability.

(4) Principal Components. Yet another widely adopted method of factorizing is one called that of "Principal Components". It was contributed by Hotelling [2] and by Kelley.[3] Here

> "the point representation of a set of variables (in the present case, test-scores) is employed, the loci of uniform frequency density are essentially concentric, similar and similarly situated ellipsoids. The axes of these ellipsoids correspond to the factors" (*Factor Analysis*, p. 159).

Otherwise expressed, the scores are measured off from the rectangular axes, of which the first is so directed as to be the best possible fit to the whole system of scores. The second axis is the next best possible fit consistent with its lying at right angles to the first, and so on indefinitely.

In such wise, the respective scores of the different persons in any test supply the best possible single valuation of his ability for that test.

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[1] *Factor Analysis*, p. 192.
[2] *J. Educ. Psychol.*, vol. xxiv (1933); *Psychometrika* (1936).
[3] *Essential Traits of Mental Life* (1935).

On the other hand, the method affords but scant relief from the just-mentioned great weakness of the centroid method; namely, that every factor (except the first) produces in about one-half of the tests positive values and in the other half negative ones.

(5) Agreement of Values. In this and the second chapter we have briefly indicated the chief direct methods of analysing tests or other variables into factors. Let us now compare the respective values, absolute and relative, yielded by them respectively when applied to one and the same set of data. For this purpose we will make use of those which have been supplied by and elaborately discussed by Thomson[1] (for four tests).

Thomson used the following matrix of intercorrelations:

$$\begin{array}{cccc} (\cdot 6) & \cdot 4 & \cdot 4 & \cdot 2 \\ \cdot 4 & (\cdot 7) & \cdot 7 & \cdot 3 \\ \cdot 4 & \cdot 7 & (\cdot 7) & \cdot 3 \\ \cdot 2 & \cdot 3 & \cdot 3 & (\cdot 5) \end{array}$$

Let us examine the loadings obtained by different methods:

(*a*) *"G" Saturation.* Here we use equation 21, p. xvi, Appendix, *The Abilities of Man.* This dispenses with communalities and gives the figures which appear in row (*a*) of the Table.

(*b*) *Centroid Load. Thurstone.* Although in Thomson's matrix the communalities are assumed to be known, we here follow Thurstone's usual practice of substituting for each communality the largest correlation in its column, and arrive at the figures in row (*b*) of the table.

(*c*) *Principal Components. Hotelling.* In this method unities are inserted in the diagonal cells and the figures obtained[2] are given in row (*c*).

(*d*) *Simple Summation. Burt's Original formula without Approximation.*[3] As the communalities are assumed to be known, we include them with the correlations in summing up each column, and obtain the figures in row (*d*).

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[1] *The Factorial Analysis of Human Ability*, chap. 2.

[2] *Ibid.* p. 72.

[3] *The Distribution and Relations of Educational Abilities*, p. 53.

(*e*) *Simple Summation. One Factor. Final Approximation. Burt.*[1] Here we get the figures in row (*e*).

(*f*) *Simple Summation. Two Factors. Final Approximation. Burt.*[2] Here the figures appear in row (*f*).

(*g*) *Weighted Summation. Two Factors. Least Squares Method. Burt.*[3] The figures, omitting decimals after the third place, appear in row (*g*).

Sir Cyril Burt very kindly supplied the junior author with the detailed calculations for (*e*), (*f*), and (*g*).

AGREEMENT BETWEEN DIFFERENT METHODS

Method	Loads by Different Methods			
	1	2	3	4
(*a*) G Saturation	·496	·823	·823	·374
(*b*) Centroid Load. Thurstone	·541	·811	·811	·425
(*c*) Principal Components. Hotelling	·662	·857	·857	·540
(*d*) Simple Summation (without approx.). Burt	·600	·788	·788	·488
(*e*) Simple Summation. One Factor. Final Approx. Burt	·493	·828	·828	·372
(*f*) Simple Summation. Two Factors. Final Approx. Burt	·500	·829	·829	·375
(*g*) Weighted Summation. Two Factors. Least Squares. Burt	·493	·834	·834	·370

On looking at the Table we see that the absolute values vary considerably though not excessively. But if we calculated their relative values, as indicated by their intercorrelations, these values would show themselves to be practically identical. Indeed the essential differences between the results derived from the different methods seem only to be in their respective fillings of the diagonal vacant places, and these fillings are, as we saw, quite arbitrary.

III. INDIRECT METHODS

(1) Expression in Series. So far, the advances of method displayed singularly small variety. There have indeed been three or four different methods evolved on different theoretical bases. But on actual application we have found them lead nearly to the same first factors. Accordingly, too, there is little difference in the next step, seeing that this merely consists in ascertaining whether the correlations calculated from these first factors do or do not agree with those actually observed. And if the answer is affirmative, therewith the factorization is finished. But if such an agreement is *not* reached, then diversities do arise. Originally, the further procedure had simply consisted in attributing the discrepancy to some error introduced by the presence of some further factor, group or general.

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[1] [2] [3] *The Factors of the Mind*, Appendix I.

But time brought a deeper interpretation of the case. The value of the correlation was no longer regarded as merely erroneous. Instead, it was taken to constitute, at any rate, a first approximation to the truth. It, moreover, was accepted as capable of supplementation into another and better approximation.

For example, let R be any of the observed correlations, whilst r_1 is its true value, and d_1 is the difference between them, so that

$$R = r_1 + d_1.$$

Continuing the procedure, we get

$$R = r_1 + r_2 \quad . \quad . \quad . \quad + r_n.$$

Here the r's represent a rapidly convergent series, so that R can be measured with sufficient approximation for most purposes by very few of them. An obvious analogy is presented by Fourier's harmonic analysis of periodic functions.

(2) Negative Factors. Another fundamental extension of method has been attained by introducing negative factors.

In the beginning, this addition was merely accidental and compulsory. As we saw, it was only the logical consequence of directing the first axis through the centroid. Furthermore, it was confined to a very special form; that of presenting the loadings for the different tests in two exactly equal parts, the one half being positive and the other negative. Moreover, this remarkable balance was preserved — at any rate, approximately

— by the method of principal components.

Now, originally, this form of factor-pattern was regarded as only a defect in need of elimination. Anyway, the negative character was taken to be inappropriate to factors in ability, this being conceived as something essentially positive.

Burt, however, had found negative correlations between special subjects and attributed them to concentration of interest on some subjects and lack of interest in others.[1]

And subsequently he has championed " bi-polar " factors and defines them as

> " those which can vary in opposite or antagonistic directions and whose saturation coefficients may therefore be arranged in four quadrants, two positive and two negative." [2]

" A typical example," he says, " is derived from residual correlations." It is mostly found in such conative cases as " brave-cowardly ". But it can, if desired, be extended to cognition also, as in the antithesis between tests which fall into " verbal " and " non-verbal " respectively.

But for the fullest development of this sphere of factor analysis, we may look to Holzinger. With him the name and concept of " bi-polar " is greatly extended.

> " A factor for which several of the coefficients are positive and others are negative will be called a bi-polar factor." It " is not essentially different from any other, but . . . may be regarded as measuring the negative aspect of the usual type of factor." [3]

(3) Rotation. But this is not the end of the story. In fact, according to some authorities (notably Thurstone), such factors as we have so far been considering " cannot be given scientific interpretation ".[4] Their sole use is said to consist in that of stepping-stones. But this function they perform with extreme thoroughness. As starting-point any factorization will do as well as any other one. And as points of arrival, the possibilities are boundless. In order to secure this drastic transformation, by far the most obvious and effective statistical means consists in submitting the reference axes to rotation. For hereby the factors are altered, but not the correlations. The guiding principle expressed spatially is as follows:

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[1] *The Distribution and Relations of Educational Abilities*, p. 58 (1917).

[2] *The Factors of the Mind*, p. 103.

[3] *Factor Analysis*, p. 104.

[4] *The Vectors of the Mind*, p. 92.

> " The sum of the projections upon a straight line of the segments of any broken line connecting two points is equal to the corresponding sum for any other broken line connecting the same two points." [1]

Now, to the exploitation of such multiform variation it is, that the great bulk of recent factor study has been chiefly devoted. And not unworthy of the unstinted labour thus expended would appear to be the unlimited diversity of results achieved; for few, if any, theories but can hopefully appeal to it for more or less statistical support.

Unfortunately, however, such a line of approach has " the defects of its qualities ". If, as said, it is consistent with every view, it ends by not assisting any one. It lacks the indispensable character which is distinctiveness. And in this wise the last word in the argument is not left to any definite statistics at all, but recourse must be brought rather in an appeal to claims of significance. These we shall consider in the following chapter.

(4) Oblique Axes. Akin to the preceding device of axis rotation — essentially in fact a particular case of it — is the replacement of the usual orthogonal axes by oblique ones. In such an operation, the communalities of the tests or other variables at issue are determined by the common-factor space of the preliminary pattern, and so again are left invariant (*Factor Analysis*, p. 234). Now, by this employment of oblique axes, a very great sacrifice is made of mathematical simplicity. But on the other hand, this loss can sometimes be offset by a notable simplification in the linear description of the scores to be rotated. In general, this happens when these scores present clear grouping or clustering; in such cases there can sometimes be achieved even the supreme reduction to one factor alone.

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[1] *Factor Analysis*, p. 216.

IV. FURTHER METHODS

(1) Partial Correlation. In the preceding account of the study of factorization, we have confined ourselves to the methods which have up to the present won for themselves most notice. But they do not yet appear to exhaust all the possibilities. An outstanding instance to the contrary has been contributed by the fine studies of T. V. Moore based upon straightforward employment of the theory of partial correlation.

(2) Variance. Another promising line of possible advance has been at least suggested on the principle of variance.[1]

(3) Inverted Factors. So far we have confined our account of factors to those derived from correlations between any two traits (or other variables) displayed by numerous persons. But very recently there has been an attempt for this relation between the variables and persons to be inverted; factors have been derived from two persons having numerous traits.

An enthusiastic advocate for this new procedure has been Stephenson,[2] for whom such inversion opens out to us

> "a new world of factor-analysis, new problems, new principles and new premises of its own."

Whereas a much more pessimistic note has been struck by other simultaneous investigators, who have protested that such an interchange of roles involves a confusion of functionally disparate things. Again, they have urged that to calculate the correlation of any column of tests implies that their respective scores must be averaged. But this operation, they said, becomes irrational since the scoring depends on the respective arbitrary scoring systems. Again, the proposed procedure is said to necessitate an intolerable amount of computation.

Stephenson, however, retorts that all these defects in the procedure are only due to faults in its manner of application.

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[1] R. A. Fisher, *Statistical Methods for Research Workers* (1934); and also *The Design of Experiments* (1937). C. Burt, "The Analysis of Examination Marks" (in Hartog and Rhodes, *Marks of Examiners*, 1936); and also *The Factors of the Mind* (especially chap. x) (1940).

[2] *Character and Personality*, iv (1935); *Brit. J. Psychol.*, vols. xxvi and xxvii (1936).

He claims that it has the final support of actual trial. As one outstanding instance he brings forward his study of mental types, such as those of the extro- and intro-vert.

As another instance he can quote his important studies of aesthetic preference.

V. VARIOUS

(1) Appraisal of Persons. As indicated or implied in the preceding section, mental factor analysis deals with two materials — processes and persons. Thus in the original and fundamental equation dealing with only two factors we had

$$M_{ax} = r_{ag} . G_x + r_{as_a} . S_{ax},$$

where the two r's are correlations describing (partially) the factors G and S_a in terms of the assumedly known trait process a, whilst G_x and S_{ax} supply estimates of the amounts of these same factors respectively by the person x. And, as we saw in Chapter II, (H), (I), (K), and (L) all four values were already determined in the earliest systematic exposition of the theory of Two Factors. In the present chapter, on the other hand, we have so far dealt solely with the generalization of r_{ag} and r_{as_a}, leaving untouched that of G_x and S_{ax}. But in actual practice it is G_x which is most required. We want measures for every single person separately.

On the whole, however, such individual measurement, although laborious, is straightforward enough. Fundamentally, the principle has remained, as it was from the first, that of regression. Only its form has changed from univariate to multivariate.

Even the chief secondary principle has remained unaltered, being that of weighting by maximum (or minimum) sums of squares.

Where recent work has gone ahead, however, is not in principle but rather in devising a variety of more or less acceptable labour-saving approximations.[1]

(2) Errors of Sampling. Finally, we arrive at a step that supplied the very keystone for rendering advance stable, or even genuine at all. This consists in ascertaining — and so taking into due account — the disturbances to be expected from the mere vagaries of sampling.

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[1] Cf. *Factor Analysis*, p. 278; *The Factorial Analysis of Human Ability*, chaps. 6 and 7.

The central procedure for this purpose consists, of course, in determining the "probable" or "standard" sampling error. Originally, and for many years, the lack of any such vital information was the most formidable of all the difficulties encountered. At length, however, the problem was solved — albeit imperfectly — by the device of "intercolumnar correlation".[1] And a few years later a further conception, that of "tetrad differences", or more briefly "tetrads", brought the matter to a stage that for many purposes left not very much to be desired.

These tetrads were of the form [2]

$$r_{ac} \cdot r_{bd} - r_{bc} \cdot r_{ad}.$$

Obviously, this was nothing more than a very simple conversion of the fundamental equation (B) given in Chapter II.

Unfortunately, the number of these tetrads is apt to be very great, and so the labour of computing them all becomes intolerable. A remedy was sought in determining the sampling error, not for each tetrad separately, but for the mean value of all of them.

So much for the earlier study of the error introduced in the process of sampling. What then — we have here to inquire — have been the chief advances later on? Our answer is that the aforesaid use of tetrads has been supplemented or replaced by the following triads:

$$r_{ac} \cdot r_{ad} / r_{cd}.$$

Obviously, this too is only a simple conversion of the equation (B). Moreover, just as in the case of the tetrads, so in that of the triads an escape from the toil of calculating each value separately is sought in the device of only dealing with the general mean.[3]

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[1] Hart and Spearman, *Brit. J. Psychol.*, vol. v (1912).

[2] *The Abilities of Man*, Appendix, p. x.

[3] *Factor Analysis*, pp. 122-125.

However, the preceding determination of the sampling error of the triads opens the road to that of the cardinal " residual correlation ": [1]

$$r_{ab} - r_{ag} \cdot r_{bg}.$$

We should note, however, that this last determination explicitly involves the Two Factor theory. Accordingly, there is little justification for its common employment by those authors by whom that theory is opposed.

Finally, we may remark that here, as in various other cases, the most modern and busy direction of research is not so much that of deepening or even correcting principles, as rather that of devising tolerable approximations.[2]

SUMMARY

The preceding chapter has briefly sketched the main advances achieved by factor analysis in the second quarter of the present century with respect to method.

In the first place, mention has been made of various more or less important elaborations of the fundamental principles at issue. These are partly philosophical and partly mathematical.

But more helpful practically has been the invention of new actual procedures to meet a special case where the older statistical attack ceased to be applicable.

This case was where equation (B) of Chapter II failed to be fulfilled, either directly or indirectly. The said procedures were those designated bi-factor, summative, centroid, and principal components. Their respective theoretical bases were not a little different. But their actual results remained throughout much the same.

They were followed up, however, by further and supplementary procedures which gave results not only different from one another but — it would appear — almost indiscriminately so. In this wise a gap was left which could not be filled by bare statistics at all, but stood in need rather of further considerations transcending these. To such we will now turn.

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[1] *Ibid.* pp. 128-129.

[2] *Ibid.* pp. 130-131.

CHAPTER V

ECONOMY *v.* SIGNIFICANCE

I. Statistical Economy
(1) Indeterminateness. (2) Some Types of Factor-Patterns. (3) Minimizing Number of Factors. (4) Maximizing Number of Zeros.
II. Psychological Significance
(1) Introspection. (2) Method of Agreement. (3) Method of Difference. (4) Elementary Variation. (5) Fit of Regularities.
III. Conclusion.

I. STATISTICAL ECONOMY

(1) Indeterminateness. In the preceding consideration of the advances achieved in factor analysis, we have been chiefly concerned with the methods devised. Let us now look at the main factor-patterns to which the methods have respectively conducted.

An outstanding result has been an almost boundless diversity. Everywhere we find that one and the same pattern of correlations can be described in terms of infinitely varied reference systems, each involving a different set of co-ordinates or other values that can be taken as factor coefficients.

Throughout then the problem arises as to which of the innumerable possibilities have been, or should be, given precedence over all the others.

(2) Some Types of Factor-Patterns. On turning back to view from this standpoint the patterns encountered above, we find them to have been mostly conceived in terms of the dominance of the factors that are respectively "general", "group", and "specific". But the last-named, since they enter into all types alike, are usually for convenience omitted from explicit mention.

So regarded, a pre-eminent position must be credited to the theory of Two Factors, inasmuch as its basal equations are limited to one positive general factor and no group factors at all (Fig. 1).

Passing next to the bi-factor method (Holzinger), here we encounter the single positive general factor just as before, but additionally several positive group factors, each of which overlaps with the general factor, but not with any other group factor (Fig. 2).

More complex is the type designated by Hotelling and Kelley as that of " principal axes ". Here the first factor is once more positive and general, but now each of all the remaining factors is approximately half positive and half negative (Fig. 3).

Still more complicated is the case of the so-called " multiple-factor " pattern (Thurstone). For this contains *no* general factor, but many positive and overlapping ones, as also many zeros. Also there is sometimes postulated a good " hyperplanar fit " (Fig. 4).

Very different is the picture when we pass on from all this algebraic to a geometrical representation of preferred patterns. One of these is exemplified in Fig. 5, where the dots supply vector representations of the respective test-scores whilst the numbered co-ordinates give the original factor coefficients. But the statistician does not stop at these. Instead he introduces a rotation whereby all the coefficients and therefore the factors are changed to give best fit, whereas the inter-angles and so the inter-correlations remain constant. The transformation is symbolized as follows :

$$F_2 = F_1 T,$$

where F_1 represents the initial coefficients and F_2 gives the required final ones, whilst T stands for the mathematical function involved in the rotating.

Another special case is presented in Fig. 6, where the original coefficients are taken from the orthogonal axes whilst thence the final and fitting coefficients are derived by simple geometry.

(3) Minimizing Number of Factors. Such having been the chief patterns hitherto preferred out of the innumerable types that are statistically possible, let us see what has been the principle or principles of selection. In general, they have been expressed in some such demands as that of "economy of thought", or "parsimony in the comprehension of nature", or "minimizing mental effort". More definite has been the return to the old-time scholastic rules of introducing the smallest possible number of causes: *non sunt praeter necessitatem multiplicandae causae.* And from this minimum of causes it is but an easy step to that of factors.

Herewith we at once arrive at the original or two-factor theory. For in this theory it is, of course, that the minimizing selective principle attains to its perfection: that of pure unity. On passing over to any of the other current theories, the number of factors can never be reduced to one only but at most can be shown to reach a minimum. But hereby is incurred a fatal loss of convincingness; no-one would think of accepting one theory and rejecting another merely because the former introduced, say, seven factors and the latter eight.

(4) Maximizing Number of Zeros. Faced by this fundamental difficulty, the search for statistical economy has been inclined to take a new direction; it has sought not so much the fewest possible factors in the pattern as rather the most numerous zeros. The actual criterion is formulated as follows:

I. Each row should have at least one zero.
II. Each column should have at least as many zeros as there are factors.
III. For each pair of columns at least as many traits as there are factors should have entries that vanish in one column but not in another.[1]

But to other workers in this field such a triple code and the ensuing factor-pattern would seem to be incredibly complicated and arbitrary. In endeavouring to realize it, they have experienced no economy of thought but rather the reverse.

Thurstone himself does not attempt to embark on this system of zeros in actual practice. As to what he really does do, he supplies only the following indication:

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[1] *The Vectors of the Mind*, p. 156.

"Many different methods have been used in these successive rotations, and it would be impossible to describe them all." [1]

II. PSYCHOLOGICAL SIGNIFICANCE

(1) Introspection. In the foregoing section all attempts to guide the preference of *factor-patterns* solely and convincingly on the principle of "*economy* of thought" would appear to have enjoyed little success, save only a partial one in the case of the theory of Two Factors. In all other cases we appear to be in continual need of supplementing the bare statistical treatment; we have to take also into consideration the material treated. This material here consists, of course, in the mental processes involved in doing the tests. Now, on certain points, the required information has been sought by way of unaided intuition. For instance, many psychologists have on this *a priori* ground confidently asserted that all mental abilities must by their essential nature be positive, not negative. A more recondite instance has been the common assumption that they can be adequately expressed as linear functions of the scores. Again, the further assumption has been unhesitatingly ventured that the vacant diagonal cells in the usual rectangle containing the given set of correlations can legitimately be filled out by imaginary coefficients of "communality".

Still the claims raised on behalf of this intuitive source of knowledge, however interesting from some points of view, appear unlikely to throw much light on the study of factorization from the present standpoint of empirical science. What really does manifest a dominant influence upon the conception of the mental processes involved in tests is the suggestion emanating from language. When once a test has received a name, this has generally been accepted as expressing its sole essential character. Few are the analysts who have pushed their examination beyond this merely popular stage.

And more rarely still has even an attempt been adventured to utilize the laboriously won resources of scientific introspection.

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[1] *Primary Mental Abilities.* Psychometric Monographs, No. 1, p. 72 (1937).

(2) Method of Agreement. However, even this much psychology has been, or should be, recognized as inadequate. In order to secure significance, there is need of ascertaining not only both the mental processes and also the statistics, but, furthermore, the fact of these two belonging together. To attain this end, the combination of mental and the statistical events must occur not only simultaneously, but also under repeated and varied conditions as to be fairly entitled universal.

Science requires the observation and establishment of coexistence according to general rules.

And in this fashion we are brought back to the time-honoured procedure that logicians used to designate as the method of agreement.

(3) Method of Difference. But there is yet another indispensable requirement of logic. We have to ascertain, not only that one occurrence regularly entails another one also, but also that the *failure* of the one entails that of the other.

In view of this further demand, the whole logical method has been called that of agreement *and* difference.

For an example and illustration of this further vital postulate, reference may be made to the evidence arrayed on the psychological constitution of G (see Chapter VIII).

(4) Elementary Variation. But there is still another logical procedure which, although perhaps less rigorous than those preceding, would seem to possess even more importance practically.

This is to the effect that in order to prove belonging-together — and thereby to justify any factor analysis at all — every elementary condition should be varied one at a time. For so long as two or more are varied in company, there is no knowing which is really responsible for any of the observed consequences. As an instance of this rule in practice, reference may be made to the determination of the nature of the " verbal factor " in Chapter XI.

This rule of elementary variation is peculiarly interesting in that it appears to mark the dividing line between factor studies of *two* fundamentally different types. One picks out traits of a more natural and therefore usually complex constitution. The other type of study deals rather with traits of a comparatively artificial and simplified nature.

Now, our rule of elementary variation is in general only feasible in the case of the second and more artificial type. The latter consequently seems alone adequate for laying the *foundation* of factor analysis, whereas the other and more complex is especially adapted for building up its *superstructure*.

For an explicit application of these two stages on a large scale see especially the Chicago experiments which are considered in Chapter VIII.

More usually some authors tacitly assume the one standpoint, whilst others no less silently adopt the other. Small wonder then that unprogressive conflict arises.

(5) Fit of Regularities. To conclude this chapter on the determination of psychological significance, the question may be asked as to the treatment of factor-patterns which display some or other definite regularity. Among the commonest kinds are any systems of positive or negative signs; any arrangement of clustering; any linearity, or rotation (see Fig. 5), or obliqueness (Fig. 6).

The answer would seem to be that even such cases, outstanding though they may be, do not differ in principle from what we have already seen.

They, no less than the simpler cases, cannot rest satisfactorily on any bare statistical basis. On the contrary, they more than any other cases are in need of psychological illumination.

And the means of obtaining this are apparently the same as before.

CONCLUSION

On the whole, then, the scope of the economical maxims would appear to have narrow limits. In one case alone has the parsimony attainable been so fundamental as to win conviction with little or no assistance from other supporting evidence; this is the case where a large set of correlations can be expressed in exclusive terms of a single factor. Save for this outstanding exception, the choice between the multitudinous possible factor-patterns stands in great need of further conditions other than that of pure mental economy.

Vital, accordingly, becomes the question as to what these further conditions may be. In the preceding chapter the suggestion has been put forward that they consist mainly in the following:

1. Introspection.
2. Method of Agreement.
3. Method of Difference.
4. Elementary Variation.

PATTERNS OF FACTOR COEFFICIENTS

+
+
+
+
+
+
+
+
+

FIG. 1
Two Factors

+	+		
+	+		
+		+	
+		+	
+		+	
+		+	
+			+
+			+
+			+

FIG. 2
Bi-Factors

+	+	−	+
+	+	+	−
+	−	−	+
+	−	+	−
+	+	+	−
+	+	−	+
+	+	−	−
+	−	+	+
+	+	+	−

FIG. 3
Principal Factors

+	+	+	
+			+
+	+	+	
+			+
	+	+	+
+	+		
	+	+	+
+		+	
	+	+	+

FIG. 4
Multiple Factors

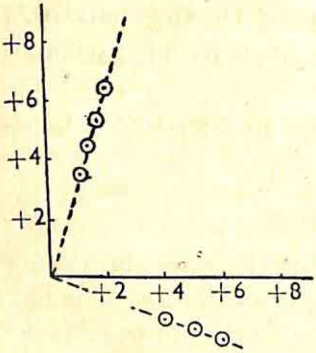

FIG. 5.—Rotation from Orthogonal to Orthogonal

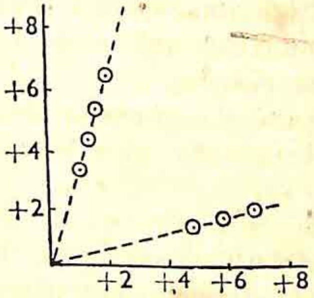

FIG. 6.—Rotation from Orthogonal to Oblique

CHAPTER VI

DEVELOPMENT OF SCOPE

I. Multiplication of Subjects
II. Multiplication of Tests
III. Foundation of "G"
IV. Delimitation of Fields
V. Quest of Completeness
VI. Refinement of Analysis
VII. Specialization of Problems
VIII. Shift of Topic
IX. The Chicago Traits

I. MULTIPLICATION OF SUBJECTS

After the preceding general sketch of the developments of factorizing methods may conveniently come a correspondingly general outline of the growth of the material to which the methods have been applied.

The most obvious direction in which such growth may occur consists in the number of subjects employed. And here, the high-water mark was touched at a far earlier date than is commonly assumed, especially by critics. In fact, no one yet seems ever to have surpassed the application of factorial methods to 2599 subjects (members of the British Civil Service) in 1923 by the senior author.[1] But at least the very tolerable figure of 1037 girls was reported by Stephenson [2] in 1931. Again about 1100 were employed in 1933 by Holzinger and the senior author at Chicago. Subsequently, most writers have contented themselves with much smaller numbers, such as 200–300.

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[1] *The Abilities of Man*, p. 153.
[2] *J. Educ. Psychol.*, xxii, 167 (1931).

II. MULTIPLICATION OF TESTS

What was thus lost by decrease in number of subjects found some compensation by increase in that of the tests to which they were submitted. Perhaps the most outstanding advance in this direction was achieved by Brown and Stephenson[1] who in 1932 not only made use of twenty different tests but, furthermore, submitted the results to mathematical treatment of previously unparalleled rigour, fundamentality, and laboriousness. Almost simultaneously, however, even all this was far out-topped by the aforesaid Chicago work, wherein the number of tests rose to no less than ninety-nine whilst the mathematical treatment was so exhaustive as to be still in progress.

Not touching this peak, but still presenting a notable record of multitude have been the fifty-six tests employed in 1938 by Thurstone.[2]

III. FOUNDATION OF "G"

Contrasting with this early, and since unsurpassed, great extensiveness of the statistical material was the original comparatively narrow theoretical quest. This consisted in little more than the discovery of tests which satisfied the hierarchic equations (Chapter II). There was no hope, nor even desire, of complying with them universally. The only question was to whether or not any tests could be found or devised which did satisfy them; at any rate, sufficiently to supply an adequate statistical basis for determining a unique G.

IV. DELIMITATION OF FIELDS

Very soon, however, this original narrow scope was far exceeded. Having first ascertained that, at any rate, *some* region of test was sufficiently hierarchic, the course of further research was to determine its boundaries. The quest was no longer confined to cases where the equations could certainly be satisfied. On the contrary, it now proceeded to examine just those cases where this satisfaction seemed most interestingly dubious. As being such were particularly chosen most of the time-honoured "faculties". Were or were not, for instance, the hierarchic equations valid right up to the extreme limits of everything legitimately included within the concept of "imagination", but no further? Analogous questions were asked about "memory", "reason", "attention", and so forth.

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[1] *J. Educ. Psychol.*, vol. xxiii (1933).

[2] *Primary Mental Abilities* (1938).

In best accordance with this doubleness of inquiry there ensued a divergence of results. The earlier ones, as for instance those recorded from the work of

> "14 experimenters on 1463 men and women, boys and girls, sane and insane",

present an almost unbroken series of compliant hierarchies.[1] Whereas the subsequent work (also recorded in *The Abilities of Man*) show compliance in only about half the cases.

V. QUEST OF COMPLETENESS

In due course, ambition pushed further. The fields to be delimited were chosen more and more extensively and systematically. They even began to aim, if not explicitly, at any rate implicitly, at covering the whole sphere of cognitive abilities.

But such a universal project can scarcely avoid involving the general theory of cognition. Hereupon it encounters the obstacle that different psychologists have widely diverse and even sharply conflicting theories. Their views, which in their general psychologies make such a brave show, are, when it comes to measuring individuals, left almost wholly out of account.

To escape this reproach the senior writer for his part has invoked the "noegenetic" doctrine which, be it right or wrong, claims at any rate to cover the whole ground with tolerable completeness. In particular, it makes contact with the entire system of psychological laws, both qualitative and quantitative.

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[1] *The Abilities of Man*. p. 138.

VI. REFINEMENT OF ANALYSIS

The foregoing development of factorization has been by way of building up the tests and factors into larger and larger wholes. But such a policy (as has been elaborately shown in *Psychology Down the Ages*) inevitably has a counterpart in breaking them down into smaller wholes. Common sense leads off with units of conveniently medium size. Science thence proceeds, on the one hand macroscopically; on the other, microscopically.

Accordingly, recent research has presented such forcible complaints as the following: [1]

> Older "work, valuable as it was, did not provide material suitable for determining the psychological characteristics of practical ability. What were the special aspects of the test that brought it into play? Such questions could only have been answered if tests had been used to isolate each aspect in turn."

Finally, to check up and fructify all such analysis, it is followed by re-synthesis; from the study of the simple processes, there is a return to that of the more complex ones (including those familiar in ordinary life). Typical of the former or analytical procedure is the finding of persons to suit known vocations. Typical of the synthetical procedure is the supplementary but harder finding of vocations to fit known persons.

VII. SPECIALIZATION OF PROBLEMS

As yet another general trend of factorization — not without intimate relations to the two preceding ones — is what may be called the increasing specialization of problems. Where the early optimistic pioneers hoped by means of very few researches to settle most of the business, later disillusionized workers often found quite a long series of investigations needful to clear up some single point. As an outstanding example, take the long and exacting series of studies of the single factor variously designated as " M ", " K ", " F ", and otherwise.

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[1] Slater, *Occupational Psychology*, vol. xiv, No. 1 (Jan. 1940).

VIII. SHIFT OF TOPIC

A further, and this time often confusing, tendency of factorizing consists in a change of its general topic. Originally and principally it has dealt wholly and solely with cognition. But sometimes — and in waxing degree — it has also dealt with feelings, impulses, and other " orectic " fields. And occasionally it has gone much further still and has tried to cope with all factorization whatsoever.

Now all these topics are, in themselves, no doubt, perfectly legitimate objects of scientific inquiry, but mischief may ensue when they are confused together, and statements only true of one are illegitimately applied to another.

IX. THE CHICAGO TRAITS

The latest, and much the most extensive, investigation bearing on our general theory was conducted at Chicago under the auspices of an International Committee with Thorndike as Chairman. It contained no less than ninety-four different tests and the subjects consisted of 1100 children, 10–17 years of age.

In order to select these tests judiciously and impartially, an appeal was made to the leading psychologists in every country. They were requested to indicate what they took to be the chief human powers of knowledge. And all suggestions made by them were taken into full and sympathetic consideration. A brief account of these tests has already been given[1] and they will be considered in some detail in Chapter VIII.

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[1] C. Spearman, *Psychology Down the Ages*, ii, 224 (1937).

CHAPTER VII[1]

EXISTENCE OF "G"

I. Problem
    (1) Metaphysics. (2) Vicissitudes of Research. (3) Conditions for Proof of Existence.
II. Crucial Experiments
    (1) Contribution of Stephenson. (2) Contributions of Brown and Stephenson. (3) Contribution of Alexander. (4) Contributions of Holzinger and his Partners.
III. Various
    (1) Further Acceptances of " G ". (2) General Factors other than " G ". (3) Absence of General Factors.
IV. Upshot

I. PROBLEM

(1) Metaphysics. In Chapters VII and VIII we are concerned with the reputed fact that cognitive ability discloses to observation a general factor, especially the one which has been designated as G. And for convenience we will treat two main questions very distinctly. Does such a factor really exist? If so, what is its nature? Taking the former question first, let us clear the ground by repudiating all the metaphysical implications that some authors have sought to read into the terms *really* " *exist* ". Such topics belong, it is believed by the senior writer, not so much to empirical as rather to philosophical psychology, with which we need not here trouble ourselves. (He has treated it freely elsewhere, as in *Psychology Down the Ages* (1)). For us, the question as to the observation of G reduces itself to that of validity of certain " hierarchic " equations. We have only to inquire whether or not these have been sufficiently well satisfied for the purpose of determining and stabilizing the factors at issue.

(2) Vicissitudes of Research. Now at first sight this inquiry would seem to have suffered a notable set-back. For in the earliest review of the evidence (quoted in (2), chap. x) a complete list of the twenty researches then available has shown *all* of these to have satisfied the said equations. But then the wind shifted. More and more frequently cases were reported of the equations failing. Louder and louder grew the voices of criticism.

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[1] References are listed at the end of Chapter VIII.

But to these again came vigorous replies. The change of results was attributed to change of attitude. For whereas the earlier workers had expressly endeavoured to find conditions that would satisfy the equations, the later efforts had aimed rather at finding those which fall short of doing so. Science, obviously, needs information of *both* sorts.

(3) Conditions for Proof of Existence. Let us first, then, begin by indicating what conditions are really essential. First of all, as already mentioned, the correlations at issue must display a marked tendency to "hierarchy"; they must, that is to say, tend beyond the range of mere chance, or error, to satisfy the following equations:

$$\frac{r_{ap}}{r_{aq}}=\frac{r_{bp}}{r_{bq}},$$

and therefore also

$$r_{ap}\,r_{bq}-r_{bp}\,r_{aq}=0.$$

When, but only when, these equations are fulfilled, then all the test-scores or other variables can be reduced to terms of a single one.

As next condition — and one much more likely to be overlooked — every tetrad stands on its own basis. The hierarchy need not be displayed by *all* the tests at issue, but only by a minimum of four of them. This is expressed in the following equation:

$$r^2_{xg}=r_{xy}\,r_{xz}/r_{yz}.$$

Here the triplet of given correlations on the right serve conjointly as "reference values" or "jumping-off ground" for the G coefficient on the left. Any further given triplets can only supply corroboration or, at any rate, correction.

As yet another condition of the factorization, it must be borne in mind that even the most careful experiments are liable to suffer from various disturbances that require amendment. In such case, the required corroboration is afforded, not at all by the raw values, but wholly by the amended ones. Here come pre-eminently the disturbances that arise from sampling and whose magnitude is indicated by the "probable error".

II. CRUCIAL EXPERIMENTS

(1) Contribution of Stephenson. Now what has actually been contributed in these fields since our landmark, *The Abilities of Man*, in 1927? As among the very first achievements of the kind we may take the research of Stephenson (3). For this work effected — besides other things — an extraordinary reduction in the sizes of the errors involved. First and foremost came those of sampling, which, of course, depend chiefly on the number of experimental subjects. Of these he managed to include no less than 1037, whereas the majority of previous experimenters had only had about 100 each. Another trouble affecting most of the older work, but happily escaped by Stephenson, had been that of excessive heterogeneity, especially in respect of sex and age. To obviate this influence, all his subjects were girls mostly between 10 and 12½ years.

Yet another danger had arisen from the arbitrariness and diversity of the scales in which the various test-scores had been respectively expressed. This disturbance he met by converting all the scales employed into such as to give standard normal distributions. And similarly all other known sources of error (4) were reduced to their minimum magnitude. Where feasible, the disturbers were eliminated. And when this could not be done, they were at least taken duly into account.

By all this corrective procedure, an extremely high standard was set up which had to be attained before the hierarchic equations could be admitted as fulfilled. Yet even so, they were successful thirteen times out of fourteen. And the author goes on to say that:

> "Neglecting this single (and readily explained) specificality gives an observed probable error of 0·0081 for a theoretical error of 0·0095."

Indeed the author goes on to conclude that his results show "good agreement" with the "Theory of Two Additive Factors".

(2) Contributions of Brown and Stephenson. Great, however, as was in many respects the superiority of the foregoing research over all previous and even most subsequent ones, there still remained other respects not yet immune from criticism. In particular, one reproach made was the absence of evidence as to how the distribution of the tetrad differences is related to the whole error curve.

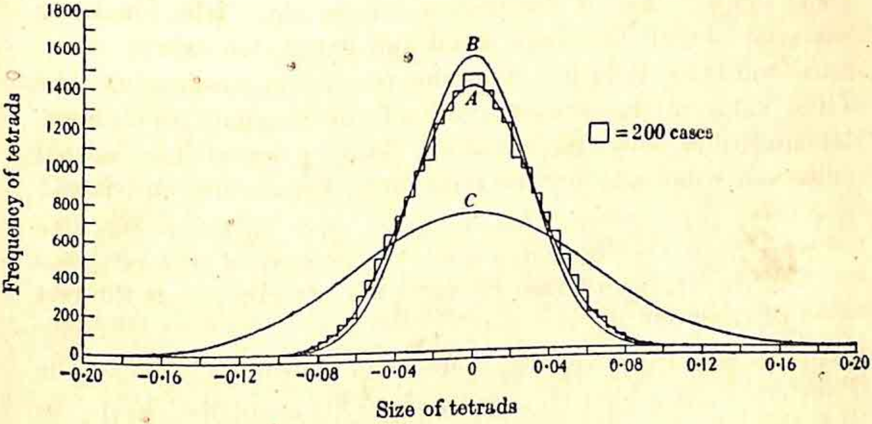

FIG. 7.—Size of Tetrads

(*A*) Curve of the tetrads actually observed. (*B*) Curve of the tetrads to be expected from the theory of Two Factors. (*C*) Curve to be expected from theory of "sampling". Clearly, (*A*) agrees very closely with (*B*) and not at all with (*C*). (Reproduced by permission from *Nature* (1934), cxxxiii, 724.)

Another objection was that the main part of the experiments had dealt with too few tests, only six; whereas there were now said to be needed some twelve to fifteen. On both counts the most emphatic critic was Pearson. Much more guarded was William Brown. By the happiest arrangement, however, Stephenson and Brown proceeded to join hands (5). In a further investigation the latter made himself responsible for the mathematical arguments and conclusions, including the curve-fitting. His partner devised and applied the tests, besides calculating the correlations and the tetrad differences.

This time, whilst the subjects only sank to the still very considerable amount of 300 boys, the number of tests sprang from six to no less than twenty. These were spread over as wide a field as possible. The set was composed as follows: 9 were "primarily perceptual", 5 more, though "not perceptual" were still "not verbal"; another 6 did have "a verbal constituent", but this was eliminated by the method of partial correlation. In this way, the number of correlations rose from 15 to 170. As for the tetrad differences, these came to no less than 14,535. As for the results, the whole of this immense material — with two very small and explicable exceptions — was found to satisfy the hierarchic conditions admirably. The chief values at issue were those of the standard deviations. Theoretically this amounted to ·028, whereas the actual observed value was ·031. Accordingly, Brown himself writes:

> "We may therefore conclude that, so far as this one frequency-constant (*i.e.* 0·031) is concerned, the criterion in the Theory of Two Factors satisfactorily passes the test of experience."

And to this irreproachable agreement of the main algebraic statistics was added an equally close fitting of the curves, as may be seen in the curves in Fig. 7 (see (6)).

In conclusion Brown himself ended by writing:

> "The main purpose of our research has been achieved, namely to establish Prof. Spearman's Theory of Two Factors on an adequate statistical basis."

(3) Contribution of Alexander. A couple of years later appeared a third outstanding work, that of Alexander (7). But whereas the other two had been mainly conspicuous for their exactitude, this one excelled principally in respect of comprehensiveness. Thus the subjects presented the utmost diversity of age, sex, and social standing, these characters being, furthermore, no longer thrown together, but instead distinguished and contrasted. As regards method too the hierarchic and the centroid were both utilized. Again, instead of the previous dominance of the scholastic verbal factor, we now find a more adequate appreciation of the factor he calls "practical". Moreover, all this experimental investigation was rendered pregnant by virtue of being framed in its educational, industrial, and social context.

What then has been the issue of this unusually extensive and judicious research for our present problem, that of finding G? The author concludes definitely enough:

> "In the Spearman tests of G we have measures of the common factor, not merely of a battery of tests, but of the whole cognitive sphere. That being so, we are more than happy to accept the existence of G as meaning the existence of 'the common intellective factor.'"

(4) Contributions of Holzinger and his Partners. Let us turn to a fourth cardinal contribution to our present problem. It exemplifies the recent tendency to extremely wide range. As regards its authorship, this was very composite: the financial and moral inspiration came from Thorndike; the tests, nearly a hundred, were planned by the senior writer; they were administered by several selected experts to about eleven hundred children; and all the results were elaborated statistically in nine successive booklets by Holzinger, of which the last appeared in 1936.

In this mammoth research, there was even less endeavour than before to obtain any ubiquitous fulfilment of the hierarchic equations. For, on the contrary, just those tests had been picked out or constructed which were most interesting on account of their fulfilment being most open to doubt. Moreover, use was here made of the new "bi-factor" method, which expressly implies the fulfilment of only a certain number of the said equations, not of the remainder ⁽⁸⁾. Accordingly, in the former cases (though not in the others), the observed median and the calculated probable error turned out to have exactly the same value.

Not content even with this vast work, however, Holzinger — this time partnered with Swineford — published in 1939 another one which carried our problem a step still further ⁽⁹⁾. This time the main character to be called into question about all these factors was the vital one of stability. This point had already been raised in the big work just mentioned, and it had been already answered in the affirmative. An analysis made of sixty-three tests had revealed nine common factors. Then thirty-six of the tests had been selected for re-analysis by the same method and the two sets of results had proved to be — with certain exceptions — in very close agreement. Turning now to the newer work of Holzinger and Swineford, this time the stability was tried still more highly. On this second occasion several new tests had been introduced. Moreover, the subjects on the two occasions were different (though of the same general type). The issue was again on the whole positive. The authors came to the conclusion that

> "remarkable stability was found for the same test battery and the same objective method of analysis."

From these same two authors, however, came yet another fundamental research (10). This time, the matter of inquiry was not so much theoretical as practical. They wished to examine their factorizing method as a "working tool". For this purpose they wanted to bring the test-scores into relation with various school studies, life occupations, and character traits. Above all, they desired to ascertain how far the reliability and the validity of the results are affected by reducing the number of tests from twenty-eight to thirteen, and therefore correspondingly economizing the time of their administration. The subjects numbered 457, about half being of each sex. On all the major points, the results of Holzinger and Swineford — guided as they were by a long succession of previous preparatory studies — showed the factor G to possess the desired stability and consistency in full measure. The general conclusion, once more, was affirmative. As in all the other researches quoted above, so here too was found a consistent and reliable G.

III. VARIOUS

(1) Further Acceptances of "G". Among yet further endorsements of G an interesting group is presented by those who, like Kelley (11) and even Thomson (12), have been depicted as arch opponents of G, whereas really they had accepted it definitely enough and had only preferred their own special interpretations of it.

> "On the whole the two sets of findings (Kelley's and Spearman's) are quite remarkably in harmony, the agreements being in the matter of a spatial, a numerical, a memory, and even a general factor, though this last is differently interpreted (11)."
>
> "With this view (of Spearman), the present author has always agreed, provided that G is interpreted as a mathematical entity only, and judgment is suspended as to whether it is anything more than that (12)."

Another group of authors consists of those who, like Brown, and subsequently Piéron, had originally been chary of admitting G but eventually had by their own further studies been won over to accept it (13). Even so, however, our list of the recent supporters of G as being at least a statistical entity is far from exhaustive. Among the more important cases may be quoted the following: Arsenian (14), Clarke (15), Wynn Jones (16), Jorgensen (17), El Koussy (18), Line (19), McManama (20), Monaghan (21), Price (22), Reinhart (23), Ebblewhite Smith (24), Milton Smith (25), Wright (26).

(2) General Factors other than "G". Up to now, however, our account of the factors found in ability has always included a general one. But a few authors have explicitly claimed that *their* general factors are not G at all. Turning to consider these exceptions, we must first of all discount those cases where the designation of "general factor" appears to be derived, not from the result of the analysis, but instead from its method (a practice that appears to the senior writer confusing).

> "I shall call those methods which proceed by analysing the correlation matrix taken always as a whole 'general-factor methods,' and those which partition the correlation matrix into suitable sub-matrices, and then analyse these separately, 'group-factor methods (27).'"

Passing on to the cases where the alleged general factors are so in the ordinary sense of the word, these appear to be almost confined to the two long familiar ones: either Hotelling-Kelley's "Principal Component" or else Thurstone's "Centroid". And even these two, though truly enough not identical with G, are usually, at any rate, very good approximations to it.

Accordingly, the great majority of authors using either of them — especially the centroid — usually have done only so in combination with, and so checked by, a determination of G also. Notable instances have been Alexander (7), Clarke (15), Flanagan (28), Price (22), Ebblewhite Smith (24) and Milton Smith (25).

Very different is the case of what Burt (11) has called the "bipolar factors". These do present values widely unlike those of G. Essentially, they are characterized by not being limited to positive values, but instead ranging freely throughout all values positive and negative. And if up to now they have attracted comparatively scant attention, this is because they have in truth offered little scope for use in the chief field of labour of factorization, namely, abilities, which are essentially positive. But they have quite recently been coming more and more into their proper estate, as research is increasingly turning to that other great side of human nature which is orexis (feeling and volition).

(3) Absence of General Factors. There will remain to consider, especially by way of contrast, the cases of absence of any general factor at all. The most striking feature about these is their extreme rarity. The senior writer is acquainted with only two of them. And even to these some objections have been made. The case brought forward by Guilford contains, if not a general factor, at least a very good imitation of it. The case of Thurstone, on the other hand, is definite enough, but it suffers from the reproach of having been elicited, not by any character of the actual data at issue, but only as an arbitrary *tour de force*. And even so, its very possibility was shown to have been largely due to the excessive size of his experimental errors (29).

Nevertheless, cases, even if not yet realized can at least be conceived, where a factorization without any general factor is irreproachably demonstrated, and might even be scientifically serviceable. Even here, however, the evidence has gone to show that such a factor was not so much really absent as only latent. By means of appropriate statistical procedure it could be immediately revived in full force.

IV. UPSHOT

We have marshalled the recent evidence as to whether or not the general factor designated as G may be properly said to exist. By this is meant that it presents at least the cardinal statistical characters — definiteness, comprehensiveness, and stability — which are indispensable for the purposes of science.

The answer, recorded in an immense amount of such research as that of Stephenson, Brown, Alexander, and Holzinger, has been a decided affirmative. Accordingly, we appear now entitled to proceed to what has been found about its properties.

CHAPTER VIII[1]

INTERPRETATION OF "G"

I. Fundamental Methods
(1) Blind Coefficients. (2) Original Suggestions. (3) Method of Agreement and Difference.
II. Relevant Experiments
(1) Older Work. (2) Unitary Traits Research. (3) Thornton School.
III. Primary Evidence
(1) "Intelligence". (2) Intellect. (3) Noegenesis. (4) Abstractness. (5) Combination of Conditions.
IV. Subsidiary Evidence
(1) Corroboration and Supplementation. (2) Anomaly of Spatial Perception. (3) Case of Arithmetic.
V. Conclusion
VI. Appendix
VII. References

I. FUNDAMENTAL METHODS

(1) Blind Coefficients. First and foremost among such properties we may notice that so far in our account the information about G has been limited wholly to its quantity irrespective of its quality. This holds good in particular of the vital "saturation" or "loading" coefficient r_{xg}. For this only indicates how much of the G at issue exists in X; it has been blind as to *what* exists thus much.

(2) Original Suggestions. Now in default of any better explanation, the senior writer originally contented himself with some suggestions more illustrative than informative. Thus on the psychological side, it was said, the chief influence governing a person's G-score might perhaps be his intelligence; or else his concentration, or his will power, or his mental energy. On the physiological side his score might perhaps be determined by his neural plasticity, or the state of his blood, or his neural energy. Another suggestion hazarded that of mere chance.

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[1] References are listed at the end of this chapter.

Again, the influence really operative might be highly complex, or even multiple. Turning to another aspect of the matter, the inward nature of the factor, the sought influence might be essentially causal, or else merely sequential.

(3) Method of Agreement and Difference. Few, however, if any, of all such suggestions would appear to have secured much support in the way of definite evidence. The simplest — and crudest — step in this direction is afforded when any factor to be explained and any explanatory test character are found to be highly inter-correlated. As an (imaginary) illustration the factor G might in some set of tests turn out to be highly correlated with the test character of spatial discrimination. From this observation the inference might be drawn that the two, G factor and spatial discrimination, are mutually explanatory. But any such inference would, to say the least of it, be precarious. For instead, the observed correlation *might* derive, not from the spatial character of the test, but rather from some other feature of it; perhaps that of being perceptual.

To guard against this contingency, more cogent evidence could be obtained by ascertaining that such a high correlation between the G-factor and the spatial character is to be found repeatedly. But even this enrichment of the evidence is not enough for security. For possibly the spatial character has in such sets of tests so very wide a range that its correlation with other characters may be extremely frequent, and therefore not really distinctive at all.

To clinch the evidence, proof may be finally supplied not only that the presence of the spatial factor and that of a high G tend to go together, but also, inversely, that absence of the spatial character entails likewise the absence of high G. But in this fashion we appear to have been at last brought back to the classical method of research designated by J. S. Mill as that of Agreement and Difference.

II. RELEVANT EXPERIMENTS

(1) Older Work. From the description of the method by which we propose to treat our problem, let us now turn to the chief data available to be so treated.

Among the older studies that might at first sight be considered for the purpose, the earliest in the field would appear to have been the outstanding research of H. Peterson (30), who in 1908 submitted sixty-three university students to five tests entitled respectively: Reasoning, Generalization, Abstract Thought, Memory, and Accuracy. For its results, see Table I. Among other distinguished works in the same sphere may be specially mentioned those of Burt (31), Stockton (32), and Herring (33).

TABLE I

CORRELATIONS OF PETERSON

Nature of Tests	1	2	3	4	5
1. Reasoning	..	·95	·83	·40	·45
2. Generalization	·95	..	·86	·40	·28
3. Abstract thought	·83	·86	..	·64	·48
4. Memory	·40	·40	·64	..	·31
5. Accuracy	·45	·28	·48	·31	..

Still, on closer inspection, none of these studies, however valuable for their own needs, appears to have supplied just the factorial data which we are now requiring, namely, data which would enable us to apply the method of Agreement and Difference.

(2) Unitary Traits Research. Passing on to recent times, we come upon the extremely ambitious work called from its topic that of "Unitary Traits" (34). This study has already been mentioned by us above, as contributing to prove that G exists. It will be here cited again, this time for the information it supplies as to how this same G may best be explained. For this latter purpose it is especially suitable on account of its extremely wide and systematic range of cognition apprehended. For these characters render it exceptionally amenable to our chosen method of Agreement and Difference. To begin with, the tests were devised in two main classes. Of these the first was primarily of theoretical significance, and so aimed at the greatest simplicity and fundamentality. The other class aimed more at contact with ordinary life, and so was for the most part inclined to be more complex; the tests were intended to serve the double purpose of verifying the theoretical conclusions and of utilizing them. (For list of traits, see Appendix at end of this chapter.)

The former or theoretical tests, (*a*), were in part directed towards salvaging whatever might be of value in the time-honoured "faculties". Groups of tests were devoted respectively to studying Perception, Intellect, Memory, Imagination, Attention, and Voluntary Movement. Each of these, again, furnished material for sub-groups. For instance, the Perception group was given one for sight, one for sound, and so on. Again, the Intellect group included Reasoning, Abstraction, and Generalization. Memory was partly "logical" and partly "mechanical"; again, partly "immediate" and partly "delayed". In the case of Imagination, the sub-groups included "controlled" and "free"; also "visual", "auditory", and "verbal".

But in addition to all such guidance of the tests along the lines of the ancient doctrine of faculties, others still more important took into account the chief modern theories of cognition, notably the one which is based on the idea of noegenesis.[1] Thus the perceiving of relations was contrasting with the inventing of "correlates". Further, the different kinds of relations were compared with one another. Yet more tests were so constructed as to illuminate the various quantitative characters: clearness, complexity, and speed; span, retentivity, fatigue, conation, and "primordial potencies"; also perseveration and oscillation. Finally, numerous tests were specially devised to elucidate not so much the essential purport of the tasks involved as, rather, certain more or less accidental influences of the manner of presentation and execution.

Then came the second general class of tests, (*b*), that by which it was hoped to keep nearer to ordinary life. These supplied three large groups which may be roughly designated as Scientific, Artistic, and Psychological. The first of these, again, presented three main sub-groups for Arithmetic, Geometry, and Mechanics; and these, especially Mechanics, were then further divided. Turning to the artistic tests, these were in the first place subdivided into literary, graphic, and musical. The further grouping of the literary tests distinguished Comprehension from Expression. The former, again, dealt separately with letters, grammar, composition, and still higher integrations. An approach to the psychological ability was sought by tests dealing respectively with emotion, behaviour, and society. Interesting additions were tests of information, as also some of the tests commonly employed to determine "Intelligence Quotients".

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[1] This word has been coined out of the Greek *nous* and *genesis* to designate the creation of knowledge in its two chief forms, the eduction of relations and that of correlates. For further details see the senior writer's *The Abilities of Man*, pt. 2, chap. xii.

(3) Thornton School. Out of the preceding work on "Unitary Traits" there were successively developed by Holzinger, Harman, and Swineford three other studies, closely akin. These, though far from emulating its grandiose scope, possessed instead continually increasing technical exactitude and rigour. For our present purpose, however, we need only consider the last of the three; the one which was executed at Thornton School (34). For this was in full agreement with the other two, besides having many more subjects (457), as compared with the Pasteur School (156) and the Grant-White one (145). As regards number of tests, all three later schools had about two dozen, and so much fewer than the Traits research.

III. PRIMARY EVIDENCE

(1) "Intelligence". After all this clearing of the ground, the actual solution of our problem becomes simple enough. It consists, as we have seen, in satisfying a two-fold condition. The first clause is that there has to be discovered some character such that its dominant presence in any of the tests at issue entails always a high loading of that test with G. The second clause is reversed; now the *absence* of the said dominant character must always entail only a *small* G-loading. Our plan of operations will usually consist in a try-out; we will take a few of the characters that seem to be, at any rate, tolerably plausible.

One such suggestion might conceivably be in the character of "intelligence". But this at once breaks down because of its equivocality. How can we even begin to see whether "intelligence" is present or absent in the tests so long as nobody knows what this word is really intended to mean?

(2) Intellect. Far superior in this respect of definiteness, at any rate, is the otherwise kindred concept of "intellect". For about the significance of this term there is comparatively little dispute, so that we can at least give it a trial.

Now commencing with the positive clause of the two-fold condition, we do find this satisfied well enough; all tests dominated by the concept of Intellect in any of its acknowledged forms do actually show high G-loadings. In the case of the results of Peterson, this agreement is very plain. Passing on to the results of the "Unitary Traits" study, the principal tests at issue are (11) Evidence relations, and (82) Generalization (see Appendix at end of this chapter), and the loadings of these respectively show the high ranks of 7th and 2nd out of the total of 41. Similarly striking are the results yielded by the Thornton School. For here the outstanding intellectual tests are (23) Deduction of letters, (25) Problem reasoning, and (28) Deduction of figures. And their respective ranks, out of 28, are as high as 4th, 3rd, and 5th.

Quite otherwise, however, are the results when we pass from the positive to the negative clause of the condition; from that of agreement to that of difference; from the "intellectual" cases where the G-loading ought to be high, to the non-intellectual cases where it ought on the contrary to be low. To the latter category belong first and foremost all the tests of sensory perception. And yet far from these all being low, one of them in the "Unitary Traits" results takes the highest rank of all the forty-one. A still more fundamental consideration in failure to satisfy the negative clause is that in all cognition legitimately termed "intellectual" the crucial relation is always that of evidence; that is to say, something is taken to "prove" something. But the high loadings, far from being confined to this evidential kind, appear to be just as high with relations of any of the other nine kinds. In the "Unitary Traits" results compare for instance the tests (11) and (13). The former explicitly tests the relation of evidence; the latter that of likeness. Yet the latter has, if anything, the higher loading of the two. If we turn to the Thornton School, the picture is, if possible, even more conclusive. High as we have seen to be the loading of the evidential tests (23), (25), and (28), at least as high are many of the arithmetic, which depend on the relation of "conjunction", (24), (26), and (27).

(3) Noegenesis. Yet another plausible suggestion would be that of noegenesis. For this does satisfy the negative clause; absence of dominant noegenesis obviously does entail absence of large loadings. But on the other hand, it breaks down at the positive clause; presence of dominant noegenesis does *not* necessarily involve presence of large loadings. An example of such failure of the positive clause is afforded by most tests of sensory perception. Thus in the "Unitary Traits" study the tests (19) Pitch discrimination, and (6) Brightness discrimination, are definitely noegenetic and yet their loadings are so low, as to have only the ranks of 27th and 21st out of 28. With the Thornton School, similarly, the noegenetic perceptual tests (2) Cubes, and (3) Paper formboard, have only the low ranks of 18th and 20th out of the 28.

(4) Abstractness. A more promising candidate to fit the bill might seem to be the character of abstractness. But on actual trial this too shows itself to satisfy only the negative clause, not the positive one; in general, the latter breaks down whenever the abstract idea is merely reproduced, not educed. The "Unitary Traits" study supplies an extreme example in test (87); this deals solely with abstract ideas, but is only reproductive, and so has a load little over zero.

(5) Combination of Conditions. But the preceding suggestions, although singly so inadequate for the satisfaction of our two-fold condition, may yet conceivably be able to cope with it in combination. Let us give them a trial, taking in particular the combination of noegenesis *with* abstractness.

Commencing with the positive clause, we find that the

"Unitary Traits" study shows the combination to be present in six of the tests, (11), (13), (17), (18), (78), and (82). As for their ranks, these (arranged in order) are 2, 3, 6, 7, 8, and 10. That is to say, *all* lie on the uppermost quartile; the agreement is flawless.

Turning to the Thornton School, here the results are still more striking. In this school, again, six tests present the said union of noegenesis with abstractness; they are (26), (24), (25), (23), (28), and (27). On the other hand, their respective ranks prove to be 1, 2, 3, 4, 5, and 7; that is to say, once more all are within the uppermost quartile.

We turn to the negative clause of the condition of correspondence. Do all the tests which *fail* to present the union of noegenesis with abstractness fail also to have high loads? This time the agreement is not quite so perfect. For although the "Unitary Trait" study presents nine tests which fail to present the said union, only seven of these fail also in respect of the loads, these having the ranks of 29, 27, 21, 40, 39, 41, and 36. The remaining two tests, (2) and (3–4), have on the contrary the very high ranks of 1 and 4.

Moreover, the Thornton School produces just the same results. Out of six tests which fail to present the required combination, only five fail also to have high loads; their ranks are 18, 20, 12, 16, and 14. The remaining case, (1), ranks as high as 6. Moreover, this exceptional test is of just the same kind as before; namely, the perception of spatial relations.

On the whole, then, this combination of noegenesis together with abstractness does satisfy the G-loadings *almost* perfectly. There remains but a single exception; this, however, is manifested in both our two sets of results, those of the "Unitary Traits" study and of the Thornton School; it derives each time from this remarkable test of spatial perception.

IV. SUBSIDIARY EVIDENCE

(1) Corroboration and Supplementation. In the preceding section we have selected for trial what seemed to be the chief claimants to constitute the required psychological characterization of G. Our method aims to be the best possible; that of "agreement and difference". And the quest may be said to have been successful. With one single exception, the whole of the available data have harmoniously indicated that G combines abstraction with noegenesis.

The present section is to be more of a corroborative and supplementary nature. It aims at gathering and focusing any illuminating side-light, so that nothing helpful should be overlooked. In particular, the fullest possible recognition should be accorded to any serious difficulties.

(2) Anomaly of Spatial Perception. Outstanding among all such troubles is the one we have just encountered about spatial perception. It is the fact of finding this ability, in spite of *not* being abstract, nevertheless giving at least one very high G-loading.

Nor can this paradox be attributed to any kind of accident or chance. For besides being much too large for such an explanation, it has a long history behind it. Originally, this test was invented by the senior writer for the very purpose of measuring G. Subsequently, it was successfully employed for the same purpose by many authors, notably Zubin and Lorge. An avowed development of it has been perhaps the best of all non-verbal tests of G, namely, the "Matrices" of Penrose & Raven (see (35)).

Along quite different lines, but with fully concordant results, has been the work of a great many other psychologists, Stephenson, Slater, and many others, on the spatial *group* factor.

But most explanatory, and fundamental, of all would appear to be the evidence of introspection. The senior writer happened to notice that such tests can readily be performed in two distinct manners. One may be called analytic, in the sense that attention wanders from one element of the figures to another. The other mode of operation is comparatively synthetic, in that the figures (or their constituents) are mentally grasped in much larger units (sometimes called "wholes"). The former procedure, not the latter, tends to load noegenetic processes with G.

As for the testee's choice between the two modes of activity, this would appear to depend on various influences, such as habit, efficiency, sense involved, and so forth. The role played by the analytic procedure may be comparatively small, but it appears — with the human mind, at any rate — to be never completely absent from anything likely to be employed as a test.

Incidentally, here may lie the real ground for the otherwise strange fact that G-loadings are almost always found in some degree, not only throughout all tests of sensory, but even in those which are commonly taken to be of a psycho-motor, kind.

(3) Case of Arithmetic. Another case that may be singled out for special elucidation is that of arithmetic. For this ability there were five tests at the Thornton School. And of these, as we may see, three stand at the very top of all, the ranks being no less than 2nd (Numerical puzzles), 3rd (Problem reasoning), and 1st (Series completion). Indeed, the authors themselves were so much impressed by the last of these, that they proposed in measuring G to drop everything else and to use this test by itself. Be this as it may, certainly these high loadings are in the best agreement with both of our requirements. For all three tests are plainly enough noegenetic. And what can be more " abstract " than number?

But a great change comes over the scene when we turn from these three tests to another arithmetical one (12) entitled Addition. For now the rank drops from the very top to almost the bottom (22nd). However, the upset is not surprising, seeing that this addition test, besides being largely one of speed, consists in nothing but continual repetitions and so is *not* noegenetic.

There remains one more arithmetical test done at this school; it is the " Fundamentals " of Woody and McCall. Now this as regards its psychological character is definitely more repetitive and mechanical than the first three considered above, but less so than the fourth, which is Addition. Accordingly, its rank too is intermediate (7th).

Turning next to the " Unitary Traits " study, here one of the arithmetical tests was again the Fundamentals. And its results were much the same as before.

Finally, we reach two more arithmetical tests entitled

Proportion and Permutation. But these were frankly experimental. They only aimed at presenting the opposite extreme to all the others in that they alone did not break up into aggregates of very familiar components, but instead constituted most unfamiliar problems. Though not really more difficult than the other tests, they made the impression of being more "abstruse". The interesting results of this change of character is that now the G-loadings were much *diminished*.

V. CONCLUSION

In general, this chapter has been devoted to bridging over the factorial gap between statistics and psychology. The principal data for this purpose have consisted of the "saturation" or "loading" coefficients. The principal method of procedure has been that of "Agreement and Difference". And the principal conclusion has been that G is essentially characterized by the combination of noegenesis with abstractness.

Among our more special topics, the most important have concerned the G-loading of spatial perception and of arithmetic.

VI. APPENDIX

"UNITARY TRAITS" (34)

(*a*) THEORETICAL UNITS

Test No.	Loading	Rank	Description of tests
			Perceptual Relations
1	·542	29	Spatial relations, I
2	·943	1	Spatial relations, II
3–4	·780	4	Spatial relations, III–IV
5	..	..	Pitch discrimination
6	·600	21	Brightness discrimination
			Perceptual Correlates
7	..	..	Perceptual correlates indicated abstractly
8	·701	12	Perceptual analogies
9	..	..	Brigham spatial, I
10	..	..	Brigham spatial, II
11	·734		Evidence relations, I
12	..	..	Evidence relations, II
13	·769	6	Likeness relations, I
14	..	..	Likeness relations, II
15	..	..	Real relations, I
16	..	..	Real relations, II

(11) Kelley, T. L. (1928). *Crossroads in the Mind of Man*, p. 22. Oxford University Press.
(12) Thomson, G. H. (1939). *The Factorial Analysis of Human Ability*, p. 240. University of London Press.
(13) Brown, W. (1932). " The Mathematical and Experimental Evidence for the Existence of a Central Intellective Factor (*g*) ", *Brit. J. Psychol.*, xxiii, 171.
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CHAPTER IX

BROAD FACTORS

I. Definition of Terms
II. Possible Constituents of " S "
III. Intruders
IV. " Broad " Factors
V. " Group " Factors
VI. Older Work
VII. Method of Elementary Differences
VIII. Bracketing
IX. " Relative " Measurement
X. Appendix: Group Correlation in Theory of Two Factors
  (1) Method of Partial Correlation. (2) Method of Excess Correlation. (3) Advantages and Disadvantages.

I. DEFINITION OF TERMS

TURNING from the aforesaid recent advances in the study of the general factor G to those which have been made in the study of the specific S, we may note first and foremost in the more modern literature a notable clarification of the concept of " specific factor " itself. This term has had at least three very different versions, which are:

(*a*) As used originally by the senior writer; that is to say, the whole content of any ability other than its general part.[1]

(*b*) The non-recurrent part; that is to say, the part of any ability which does not recur in any other of any given set of abilities.

(*c*) Any ability content which cannot recur in any other ability whatever.

In recent times there has been some improvement, in that most writers, however much they may still mix up the actual terms they use, do, at any rate, leave less doubt as to which of the meanings they intend. Usually, this is (*b*).

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[1] *J. Educ. Psychol.*, xxiv, 599 (1933).

II. POSSIBLE CONSTITUENTS OF "S"

After this preparatory clarification of the argument, and taking into due account the results obtained in the previous chapters, we arrive at the conclusion that the scope of S (in the sense (*c*)) is almost boundless. A full description of this scope will naturally vary according to the describer's general psychological theories. But at least some notion of this scope may be gathered from the very comprehensive list of tests arrayed in our account of the Unitary Traits experiments. However, even this list falls notably short of actuality. In particular, it is much biased in the direction of qualitative mental performances, and so is meagre in respect of the quantitative features such as the distribution of attention. Thus it takes little account of the fact that two individuals may be equally successful at performances involving only relaxed attention, and yet very unequal when the attention has to be strained.

Again, a further sphere of specific abilities may be invoked according to the method of presentation employed. Thus some subjects may be favoured by words presented visually whereas other persons prefer the presentation to be auditory.

III. INTRUDERS

In addition to all the preceding constituents of S-factors which may be called intrinsic, there are others that by contrast may be termed extrinsic; they are only intruders in the experimental situation. Paramount here come the disturbing "errors". These, moreover, fall into two categories: those that do, and those that do not, recur on repeated application of what is intended to be one and the same test.[1]

The latter category has been taken as representing the test's grade of "unreliability". It admits of elimination by the simple, if cumbrous, device of making the affected test longer. And even without any such actual elimination, its effect upon the correlations can be approximately removed by a well-known statistical correction.

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[1] R. H. Thouless, "Test Unreliability and Function Fluctuation", *Brit. J. Psychol.*, xxvi, 325 (1936).

In either case, we are left with what has been called the " net " or " purified " values of the specific factors. And it is purified S, rather than the " gross " or " raw " S, which we shall chiefly try to record in the following pages.

In view of this usually complex character of the specific factor, we shall find that this, unlike the general one, has been frequently divided into more or less numerous sub-factors. Increasingly common has been that which has separated off the aforesaid sub-factor of unreliability. In this fashion the standard equation becomes represented :

$$M = G + S = G + S^{1} + U,$$

where U stands for the degree of unreliability.

IV. " BROAD " FACTORS

Now the above described complexity of the situation would appear to exercise a drastic influence upon the whole study of specific factors. For if the number of independent ones is indefinitely numerous, it is hard to see how we can ever attain the often cherished ideal of expression in terms of very few.

A solution of the difficulty has been found in the fact that out of all the said specific factors only an extremely small portion cover an appreciably broad ground. The rest can for most purposes be neglected, leaving the few broad ones to supply, at any rate, useful approximations. To these " broad " factors, accordingly, have been devoted the great bulk of the following pages.

V. " GROUP " FACTORS

They must, however, be sharply distinguished from what have been called " group " factors.

The latter have been unequivocally defined as those which are present in more than one of any given set of abilities, but not in all of them. But obviously, then, they depend, not on any absolute qualities of the abilities at issue, but, instead, on the relations of these abilities to all the others which may happen to have been selected to make up the set for joint consideration.

Unimportant as this may be, the concept of "group" factors for the intrinsic description of specific factors, they remain vital for statistical manipulation. This fact has already been abundantly evidenced in Chapter IV.

VI. OLDER WORK

From the preceding remarks it would appear that the study of factors (other than the general one) almost necessarily deals with the kind which we have called "broad". But these unfortunately do not admit of precise definition. In *The Abilities of Man* they were taken to be those which possess

> "sufficient breadth and degree to have serious practical consequences, educational, industrial, and vocational."

Now the older work, in its search for such broad factors, devoted itself specially at examining all cases of two sorts. In the first place, it tried out every class of relation: to wit, those of evidence; those of likeness; of time; of space; of conjunction; and those designated as psychological. In the second place, it made assay of the chief time-honoured "faculties", which were taken to be Intellect, Sensory Ability, Motor Ability, Abstraction, Language, Memory, and Imagination.

On the whole, the findings were surprisingly negative. That is to say, the value of (N) (p. 13, Chapter II) proved to be rarely greater than was to be expected from the bare errors of sampling. And so the criticisms of the faculty doctrine by Herbart, Thorndike, and many others found justification.

But positive findings, though rare, were not by any means altogether absent. As the five most important instances were picked out the following: the logical, the mechanical, the psychological, the arithmetical, and the verbal.[1] Already these, as may readily be seen, are in excellent general agreement with the more elaborate results obtained later on. Time has not upset, but only developed, almost all the conclusions reached by the same method. As for the results obtained by other methods, these will be considered in their respective turns.

VII. METHOD OF ELEMENTARY DIFFERENCES

We arrive finally at two fundamental difficulties, to meet which the present chapter has principally been introduced. One consists in the insuperable need to make the factorial solution determinate. The other — not independent of the former — is the paramount requirement that it should be supplied with convincing psychological meaning.

Now, as we have seen, both these needs were furnished in the case of the general factor by the method of "agreement and difference", the principle being that this method involves such extremely numerous and exceptionless coincidences that these cannot fail to be significant. But this principle breaks down in the case of specific factors — even broad ones — for here the available coincidences are usually much fewer. Commonly, indeed, this trouble is disposed of lightly enough. The meaning is forthwith taken to be that feature shared by different abilities which is indicated by their respective names. Thus the factor shared by the handling of two familiar toys has been forthwith explained on the ground that both of them involve a "mechanical factor". But the factor really responsible for the correlation between success with the two toys may be much more particular; thus it might be the mere fact of the children having played with both these toys previously. In other cases, the mistake may be in the opposite direction; the really effective factor may not be more particular but more general. For a correlation observed between several tests all labelled "deduction", and, accordingly, all attributed to a deductive factor, may really derive from a factor which extends to all exercise of reasoning ability. In this embarrassment, our proposed remedy is as follows.

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[1] *The Abilities of Man*, pp. 241-242. For the verbal factor, see especially the later edition of the same book (published in 1932).

The probability of significance gained by sheer multitude of coincidences is — as it must be — sacrificed. But in its place is substituted a probability gained from the elementary nature of each coincidence on its own account.

To take an actual instance, suppose the stimulus to consist of two similar tones and the reaction to be a judgment of similarity. Next, introduce the sole complication of a long interval between the stimulus and the reaction, whereby the judgment acquires a much larger error. To derive the error from the interval would seem inevitable. And if the whole experiment is repeated in different parts of the musical scale with analogous results the cogency of the argument becomes still greater. For an almost ideal example of an analogous case, reference may be made to the study of the verbal factor in Chapter XI.

On the whole, this method of Elementary Differences would appear by far the more potent of the two. But, on the other hand, it usually demands a correspondingly more skilful experimentation, both statistical and psychological.

VIII. BRACKETING

To conclude this chapter we may note that not only the preceding two classical conditions (agreement-difference and elementary changes), but also any other well evidenced regularity is statistically equivalent to a condition imposed on the factorial solution (OVER AND ABOVE those constituted by the observed correlations). One and all — regularity and correlations — contribute to that great need for any useful factorial solution, namely, determinateness.

The utility of such a supplementary condition may be instanced in what we may call bracketing. In the first place, suppose that observation has shown any pair of abilities *not* to have some specific correlation. There the matter rests. But now introduce (on good grounds) the condition that the similarity of the pairs of correlations at issue can be graded serially. We can now infer that not only the observed pair of abilities, but *a fortiori* all less similar pairs, have not got the considered specific correlation. In this fashion, the originally observed pair supply an upper limit to the possible presence of the specific correlation.

Let, next, the originally observed pair of abilities, on the contrary, to have the specific correlation. It then analogously furnishes a lower limit to possible absence of the specific correlation. Eventually, by oscillation between upper and lower limits, the two finally meet together.

IX. "RELATIVE" MEASUREMENT

So far in this chapter we have been considering solely the group correlations obtained by means of (M), see page 12 of Chapter II. There the procedure was based on a preliminary measurement of the correlations of the tests (or other variables) with G. But here we will finally turn to the case — extremely common — where no such preliminary basis is explicitly employed. The kind of case at issue is that in which the tests are arrayed by groups, so that, in general, the correlations within any group may be expected to be higher than those from one group to another. The following is an elementary (imaginary) example, where a and a^1 belong to one group, b and b^1 to another. The situation is akin to what has been so effectively handled in the "bi-factors" of Holzinger.

	a	a^1	b	b^1
a	. .	·60	·20	·20
a^1	·60	. .	·20	·20
b	·20	·20	. .	·50
b^1	·20	·20	·50	. .

Now the simplest way of here measuring group factors is to take as criterion the excess of the mean intra-group correlations over that of the inter-group ones. Use is made of some such formula as the following:

$$E_{ab} = \tfrac{1}{2}\,(\bar{r}_{aa} + \bar{r}_{bb}) - \bar{r}_{ab}.$$

In the present case E_{ab} amounts to ·35.

Unfortunately, however, E_{ab} is usually indeterminate as two unknown quantities, the group factors, are involved.

To obviate this pitfall, recourse is rather lightly had to the excuse that each of the said factors is determined "relatively" to the other one. But this claim would appear to be unwarranted. In truth, the factors are (within large limits) mutually independent.

A more legitimate remedy may be had by adding a further group factor based on the further variables c and c^{1}. By so doing the number of unknowns is indeed still further augmented, namely, up to three. But now the equations also attain to the same number, so that all has become square and determinate. The table of correlations has got the following form:

	a	a^{1}	b	b^{1}	c	c^{1}
a	..	·60	·20	·20	·20	·20
a^{1}	·60	..	·20	·20	·20	·20
b	·20	·20	..	·50	·20	·20
b^{1}	·20	·20	·50	..	·20	·20
c	·20	·20	·20	·20	..	·40
c^{1}	·20	·20	·20	·20	·40	..

So much for the theoretical possibility of determining the magnitude of the group factors. We conclude with the practical task of actually measuring. The group factors themselves may be written in the usual manner of partial correlations as

$$r_{aa'\cdot g} \qquad r_{bb'\cdot g} \qquad r_{cc'\cdot g}.$$

These unknowns have to be measured in terms of the known quantities E_{ab} E_{ac} E_{bc}. Here we may introduce the fact that each total equation can be expressed as the sum of two partial correlations: firstly, that which is obtained by eliminating G; and, secondly, that is thereupon "residual". Employing this expression and some algebra, see appendix to this chapter, we get then finally: In example

$$\begin{aligned} r_{aa.g} &= +E_{ab}+E_{ac}-E_{bc} = \cdot 40 \\ r_{bb.g} &= +E_{ab}-E_{ac}+E_{bc} = \cdot 30 \\ r_{cc.g} &= -E_{ab}+E_{ac}+E_{bc} = \cdot 20. \end{aligned}$$

At long last we may note that all our measuring by way of these E's does not conflict with the use of (M), but only regards the same facts from a little different angle; one that may be a little more or less convenient.

X. APPENDIX

GROUP CORRELATION IN THEORY OF TWO FACTORS

(1) Method of Partial Correlation. For the clearest and most fundamental expression of group correlation, nothing would seem to surpass that which is afforded by the sums of linear functions.

As a simple illustrative case, this expression might take the following form:

$$\begin{aligned} m_{ax} &= w_{ag}.\mathrm{G}_x + w_{ap}.p_x && + w_{as}.\mathrm{S}_x \\ m_{bx} &= w_{bg}.\mathrm{G}_x + w_{bp}.p_x && \\ m_{cx} &= w_{cg}.\mathrm{G}_x && + w_{cq}.q_x \\ m_{dx} &= w_{dg}.\mathrm{G}_x && + w_{dq}.q_x. \end{aligned}$$

Here *a*, *b*, *c*, and *d* are tests (or other analogous variables); the *m*'s are the scores; G_x is a general factor, p_x and q_x are group factors; the *w*'s are the respective loadings or weights. Moreover, such an expression is only formal; it presupposes the factorial analysis, instead of showing how to obtain it. For the latter purpose, and in particular for measuring the correlations due to group factors, the technique had first to be derived from the theory of partial correlation, the full formula employed being as follows:

$$r_{ab.g} = \frac{r_{ab} - r_{ag}.r_{bg}}{(1 - r^2_{ag})^{\frac{1}{2}}(1 - r^2_{bg})^{\frac{1}{2}}}.$$

(2) Method of Excess Correlation. So much for the original method of measuring the correlation ascribable to "group" factors. Directly or indirectly, it depends fundamentally on the theory of partial correlation. Recently, however, there has been introduced another method on a fundamentally different basis. This is built on the concept of groups; it assumes that other things being equal, the correlation between tests (or other analogous variables) belonging to one and the same group will be higher than that between tests belonging to two different groups. Furthermore, it assumes that the amount of such excess will, in general, be approximately proportional to that of such difference.

Let us illustrate this second method by the same example as before, but for convenience, changing the symbols. This time we get

$$\begin{aligned} m_{a_1x} &= w_{a_1g}.G_x + w_{a_1p}.p_x && + w_{a_1s}.S_x \\ m_{a_2x} &= w_{a_2g}.G_x + w_{a_2p}.p_x && \\ m_{b_1x} &= w_{b_1g}.G_x \qquad + w_{b_1q}.q_x && \\ m_{b_2x} &= w_{b_2g}.G_x \qquad + w_{b_2q}.q_x. && \end{aligned}$$

Here the two a's, a_1 and a_2, belong to a single group, and the two b's belong to another group.

Consider the following expression :

$$E_{ab} = \tfrac{1}{2}\,(r_{a_1a_2} + r_{b_1b_2}) - \tfrac{1}{4}\,[r_{a_1b_1} + r_{a_2b_1} + r_{a_1b_2} + r_{a_2b_2}].$$

The part of this expression inside the curved brackets may be written as follows :

$$(w_{a_1g}.w_{a_2g} + w_{a_1p}.w_{a_2p} + w_{b_1g}.w_{b_2g} + w_{b_1q}.w_{b_2q}),$$

whilst the part in rectangular brackets reduces to

$$[w_{a_1g}.w_{b_1g} + w_{a_1g}.w_{b_2g} + w_{a_2g}.w_{b_1g} + w_{a_2g}.w_{b_2g}].$$

Throwing these two parts together, we get for E_{ab}, on the one hand, a sum of products involving the factor G, and on the other hand, a sum involving instead the group factor p or else the group factor q. But as may readily be seen, all the g-products tend to cancel out the plus and minus values as they tend to be equal. This, however, leaves over a substantial residuum :

$$\begin{aligned} E_{ab} &= \tfrac{1}{2}\,(w_{a_1p}.w_{a_2p} + w_{b_1q}.w_{b_2q}) \\ &= \tfrac{1}{2}\,(r_{a_1a_2.g} + r_{b_1b_2.g}). \end{aligned}$$

Put into words, E_{ab} measures the mean excess of the two excess correlations produced by the group factors p and q in $r_{a_1a_2}$ and $r_{b_1b_2}$ respectively.

But in such fashion we obtain only one equation for two independent excess correlations. And so the result is indeterminate.

However, by adding on to the groups *a* and *b* any further group, say, *c*, the equations and the excess correlations become three in number.

Accordingly, in actual fact we get on solving for the correlation excess as follows:

$$\begin{array}{llll}
r_{aa.g} = \mathrm{E}_{ab} + \mathrm{E}_{ac} - \mathrm{E}_{bc}, & \text{or} & \text{more simply} & r_{aa} - r_{ab} - r_{ac} + r_{bc} \\
r_{bb.g} = \mathrm{E}_{ab} + \mathrm{E}_{bc} - \mathrm{E}_{ac}, & ,, & ,, & r_{bb} - r_{ab} - r_{bc} + r_{ac} \\
r_{cc.g} = \mathrm{E}_{bc} + \mathrm{E}_{ac} - \mathrm{E}_{ab}, & ,, & ,, & r_{cc} - r_{ac} - r_{bc} + r_{ab}.
\end{array}$$

(3) Advantages and Disadvantages. Finally, let us consider the two methods as regards their comparative advantages. In the first place, the way of partial correlation must be conceded to excel in respect of generality of scope; whereas the way of excess correlation is usually confined to correlation patterns that have been specially constructed for it.

But on the other hand, such patterns occur very frequently, and probably they ought to be employed more often still.

In the second place, the other method, that of excess correlation, is far simpler and quicker. So much so, that even when not preferable for ultimate decisions, it may yet be most convenient for preliminary orientation.

But there is yet another point to take into account, and one of vastly greater importance still. This consists in that whereas the method of partial correlation is throughout restricted to the sphere of bare statistics, the method of excess correlation can operate statistically and psychologically in most intimate combination.

CHAPTER X

INTELLIGENCE

I. Evidence
(1) Nature of Case. (2) Older Work. (3) Contributions of Thurstone. (4) Contribution of Holzinger and Swineford. (5) Conclusion about Evidence.
II. Abstraction
(1) Problem. (2) Research. (3) Conclusion about Abstraction.

I. EVIDENCE

(1) Nature of Case. After the preceding remarks on specific factors in general, let us turn to consider the chief particular findings. Now for many reasons, both historical and essential, we are moved to begin with the notorious " intelligence ". But this in its turn involves two paramount features : evidence and abstraction. And in such fashion we are brought back to the same two characters as we were in the analysis of G. But now they are regarded by us from a very different angle. From being mutually indispensable supplements, now each of them stands on its own independent basis.

Of the two, we will commence with " evidence ". This case is that of any mental content functioning as evidence for the existence of any other such content. Here one fundamental question is as to whether there is or is not any " upper limit " where the two compared mental contents are so extremely dissimilar as to present no specific correlation whatever.

(This question, as may easily be seen, is essentially the same as that about the scope of equations (B) and (C) in Chapter II.)

(2) Older Work. For the extreme case of such dissimilarity — always within the scope of evidential cognition — we might go right back to the original work of 1904, and let the two cases at issue consist of "perception" and "inference". Then, using equation (N̊), Chapter II, we get

$$r'_{pi} = r_{pi} - r_{pu} . r_{iv} / r_{uv},$$

where *p* stands for perception, *i* for inference, whilst *u* and *v* are the required two reference abilities. Now, a little calculation on the basis of the correlations recorded in the said work of 1904 shows that in actual fact the value of r'_{pi} does approximate to zero. Far more explicit and rigorous, however, was the case supplied subsequently by Dockerill and the senior writer. Here the extreme difference between perception and inference was eliminated, both cases belonging to the latter category. But with this restriction, the two were about as dissimilar as the common character of evidence appeared to furnish. One was inference again. For instance:

> "If I can run faster than Tom, and the same as Harry, but slower than Dick, who will win the race, Tom, Harry, or I?"

The other case employed was designated as likelihood; in it the subjects had to decide whether some given statements were quite certain, only likely, very unlikely, or downright absurd. For reference values, use was made of the familiar

> "Opposites, Synonyms, Classifications, Completion, Questions, Analogies, Paragraphs, Meanings and Memory."

If now we denote inference by *i*, likelihood by *l*, and the two reference values by *u* and *v*, we get

$$r'_{il} = r_{il} - r_{iu} . r_{lv} / r_{uv},$$

which makes $r'_{il} = -\cdot 024$, which is clearly insignificant.

With the two preceding results of Dockerill and the senior author where the specific correlation proved to be about zero, let us contrast that of Peterson where it turned out to be very large.[1] Here the specific correlation at issue is that between Reasoning and Generalization.

The former may be instanced by the following problem:

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[1] *Psych. Rev.*, vol. xv (1908).

> " Giving advice is useless. For either you advise a man what he means to do, in which case the advice is useless; or you advise him what he does not mean to do, and the advice is ineffectual."

As for the test of Generalization, this is a paragraph of 21 lines beginning:

> " What general statement or statements can you make which will be true of all the following instances of the way in which Colonial schoolmasters were paid? In Watertown, Mass., in 1680, the schoolmaster got 25 pounds and the benefit of the Latin scholars."

For reference values were used tests called " Abstract thought, Memory and Accuracy ". The resulting specific correlation, calculating as before, turned out to be no less than ·821. From consideration of the nature of the two abilities, reasoning and generalization, the group factor connecting them was taken to be " logical ".

(3) Contributions of Thurstone. Of more recent investigations bearing on the factorization of " reasoning ", a prominent place must be credited to the work of Thurstone entitled *Primary Mental Abilities*. This author introduced into his sixty tests no less than ten which were expressly designated as being of " reasoning ", four being further qualified as being " numerical ", three as " verbal ", and three as " spatial ". All these titles, accordingly, must be provisionally accepted as constituting the psychological data; by according with these data it was, then, that his factors had eventually to justify themselves. As for his method of calculating these factors, see our Chapter IV. First, a set of preparatory values were obtained in the ordinary way. And then all these values were freely modified by an indefinitely large number of rotations of the reference frame. As to how these were effected, he only tells us that

> " many different methods were used and it would be impossible to describe them all."

In this fashion he arrived at thirteen factors, of which three were definitely concerned with reasoning: " induction ",

" reasoning ", and " deduction ". However, we must bear in mind that these were rather unexpectedly defined as follows:

> " Induction " makes the subject " find a rule or principle." " Reasoning " " involves some form of restriction in the solution." In " Deduction " " the subject must first find the rule and must then apply it to the given examples."

Now comes the crucial moment of checking the agreement of these factors with the tests on their respective mental aspects. To be satisfactory, all ten tests chosen as measuring the power to reason should correlate significantly with one or more of the three factors obtained for the same purpose. Inversely, no test other than those ten should correlate with any of those three factors, since these three are by assumption independent of all other factors. But actually this is far from being the case. Only five of these tests correlate with these factors. For instance, the test of " verbal reasoning " (41) only correlates with " induction " to the insignificant amount of ·138. On the other side no less than sixteen of the non-reasoning tests do correlate with the reasoning factors: for instance, " Areas " (29) correlates with " Induction " to the amount of ·477.

It may be of interest to see how these data of Thurstone fare with the method of Two Factors. On picking out all the same ten tests chosen to present reasoning, there appears to be only one instance of specific correlation, but that a very high one. It is between " syllogism " (40) and " false premises " (42) and comes to ·60. And so once more — as with the results of Peterson — we reach an outspoken case of formal logic.

(4) Contribution of Holzinger and Swineford. We may now pass on to the great work of Holzinger and Swineford, often quoted by us already. These authors introduced into their twenty-four tests five bearing the generic title of " mathematical induction ". More specifically they were entitled " deduction " (20), " numerical puzzles " (21), " series completion " (23), " problem reasoning " (22), and " computation " (24). The procedure adopted was to assume that all these tests had group factors among themselves, and then to measure these. The result was wholly negative; in every case the group factor turned out to be insignificant. What then, it may be asked, has here become of the group reasoning factor recorded above in the reasoning tests of both Peterson and Thurstone? The answer is simple. The said factor was not found in all the tests of reasoning, but solely in those of a particular type, described as that of formal logic. But in the present work there was *only one* test of this type (20); so that the opportunity for any correlation did not arise.

(5) Conclusion about Evidence. On the whole, the results, old and new, are unanimous in denying the existence of any specific factor that can appropriately be designated by any such general term as "reasoning", "inference", "deduction", "induction", or even "evidential relation". The factors actually observed either go beyond any such limitations, as occurs of course with G; or else fall notably short of them, as occurs with the repeatedly observed factor and ensuing correlation derived from formal logic. Success in this subject would seem to present a certain degree of functional unity.

II. ABSTRACTION

(1) Problem. In our further quest for broad factors, let us turn from the relation of evidence to the other great character commonly attributed to the Intellect — incidentally, found by us necessary, though not sufficient, to constitute G — That is to say, Abstraction.

Does this character — or anything closely akin to it — produce any broad and unitary function, such as we here require?

Once more we are faced by two demands. Firstly, there is that of agreement. Can we find any statistical factor such that it makes its appearance on the scene whenever any reasonable conception of "abstraction" does so? Secondly, there has to be met the difference-demand. Does the said factor *fail* to appear whenever the abstraction is absent? Here the case is no longer simple.

(2) Research. Of any such technical refinements, however, there has so far little been manifest. Reinhart[1] indeed in 1931, working under the direction of T. V. Moore, did explicitly introduce a test entitled one of abstraction. But he arrived at mention of it in his eventual scheme of factors. He designates as being " of considerable importance " only " the cognitive general factor " (9) and " the power of abstraction ".

Four years later, a factorization of cognitive ability, done under the same direction by Monaghan,[2] does not so much as trouble to allude to abstraction at all. Thurstone, for his part, however, gives us five tests explicitly designated as appertaining to abstraction. From these, moreover, he sharply distinguishes eight others called instead " verbal tests ". But in truth the distinction is hard to maintain. The abstraction tests are as follows: Reading I, Reading II, Verbal Classification, Word Grouping, and Figure Classification. Every one of these tests called abstractive might just as well have been called Verbal (except 8, which does not belong here anyhow!).

Inversely, almost every one that is thus called Verbal might instead have been called abstractive.

Even scantier would appear to have been the consideration accorded to abstraction by Holzinger and Swineford. In fact, they do not appear to allude to it anywhere.

(3) Conclusion about Abstraction. We seem cogently led to the conclusion that the character of abstractness, however vital when suitably combined with that of evidence, is singularly unfruitful when taken in isolation.

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[1] *Cath. Univ. Amer. Stud. Psychol. and Psychiat.*, vol. ii (1931).

[2] *Ibid.* vol. iii (1935).

CHAPTER XI

VERBAL FACTOR

I. Pre-correlational Study of Language
II. Reading and Perception
III. Introduction of "Factors"
IV. Reduction of Sampling Errors
V. Requirement of "Breadth"
VI. Relation of "V" to "Intelligence"
VII. Stability of "V"
VIII. Results from Other Methods of Analysis
IX. Summary

I. PRE-CORRELATIONAL STUDY OF LANGUAGE

In the preceding chapter we have considered the specific factors in a general way. Let us now turn to the study of them singly. If we would follow their strict chronological order, and thereby restrict ourselves to such as can reasonably be regarded as "broad" (see previous chapter), we ought to start with "memory" as discovered in 1906 by Krueger and the senior writer. But if we prefer to commence with that which first received general recognition, we ought perhaps to begin with the so-called "verbal" factor.

From the earliest times — long before any concept of "factors" at all — the nature and function of words had provoked some of the keenest, not to say fiercest, feuds throughout psychology. Indeed these dated right back to epistemology and even ontology, generally under the designation of "logos". And the battles only intensified when the storm-centre shifted in the Middle Ages to the analysis of abstract thought. From these giddy heights, however, the role of language in cognitive ability eventually dropped down to the level of humdrum empirical science. Here the most stimulating experience was that which came from the sphere of pathology. Here the striking observation was made that a certain class of brain lesion was apt to cause a grievous loss of linguistic ability without any corresponding affection of intelligence.

Finally, the problem was invested with vital importance for the most modern trends of psychology by leading to patently inacceptable results of applying linguistic tests of intelligence to foreigners. To remedy this embarrassment, further tests were devised for them from which the verbal character had — it was hoped — been eliminated.

These new tests were accordingly designated as "non-verbal"; or, more positively, they were called "performance" ones. Among the investigators most helpful in this direction were Pintner, Terman, Brigham, as also Healy, Fernald, and Thorndike.

The subsequent comparison between these two kinds of tests, however, showed them to have comparatively poor mutual correlation. And so Thorndike, with many others, began to talk of two separate intelligences, the one being verbal but the other not so. But this seemed to go too far in the opposite direction. For it left unexplained the undeniable fact that the two kinds of ability had, at any rate, *some* correlation with one another; they were indeed partly different, but also partly the same. The situation had become difficult.

II. READING AND PERCEPTION

The preceding trouble, however, is essentially not psychological but only statistical. We have been looking out for correlations, with but little regard for the mental entities correlated. Let us now turn to these. Among the earliest outstanding works in this region was that of L. Fildes in 1921. Her procedure was to examine how those children who "had great difficulty in reading differed from normal in various other kinds of perceptual ability".[1] Her chief finding was that such children were not appreciably inferior in recognizing visual designs, but were so in recognizing words, syllables, and digits.

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[1] The junior author has not been able to verify this reference nor those in the next paragraph.

Only five years later came the still more fruitful harvest of Gates, who studied 135 (normal) children of Grades III and VIII in respect of the correlation of reading and spelling with eight tests of visual perception; only two of these showed appreciable values, namely, "small differences of nonsense syllables" and "small differences of words". On turning to "small differences in drawing", and to "small differences in digits", all significant correlation disappeared. Nor was any found with the remaining four tests, which were cancellation of geometric figures, A's, K's, and 23. Another vital result obtained by the same investigator was got from some deaf children 4½ to 9 years of age. Words were presented to them

> "in connection with objects or pictures, or in other situations."

On being tested in reading four months later, they proved to be greatly superior to an equivalent control group

> "taught by the regular institution methods."

This is taken to indicate that individual differences in reading are not primary but secondary; they can be explained by differences in effectiveness of perception. Yet a third fundamental experiment was made by Gates. In this

> "the subject was required to learn the names associated with a figure presented visually. The task seemed to be identical with the learning of printed words."

Nevertheless, the correlations with

> "reading, pronunciation, spelling and perception were all low."

This appears to prove that the better perception responsible for the better reading is not innate but acquired; it can be credited to easier recognition due to more appropriate habits of perception previously acquired, such as making a careful visual study of words when presented and endeavouring to recall their visual appearance when studying afterwards.

Several of these results, notably the correlations, were three years later excellently checked, verified, and developed by Riley.[1] She, too, found that reading and spelling were importantly correlated with discrimination of words and almost equally with unrelated letters, but not of designs, or even of digits. The following is her chief array of correlations.

RILEY'S CORRELATIONS

Tests	Reading	Spelling	Intelligence (largely Pictorial)
1. Words .	·48	·60	·36
2. Words .	·63	·65	·30
3. Digits .	·37	·31	·19
4. Letters .	·63	·54	·31
5. Designs .	·03	·02	·06
6. Words .	·75	·75	·31
7. Digits .	·25	·26	·12
8. Letters .	·42	·47	·27
9. Designs .	·19	·23	·26

III. INTRODUCTION OF "FACTORS"

But at this stage the running in the pursuit of linguistic research was taken up by those psychologists who were developing a much newer line of investigation; they had already, as we have just seen, adopted the calculus of correlation coefficients; they now turned to that of factors.

The first to apply these to the problems of language would appear to be Davey.[2] Her experimental procedure consisted essentially in devising eight abstract and verbal tests together with six of a perceptual kind. In four cases the two sorts were so constructed as to be similar in all possible respects save only just this of being abstract-verbal or else perceptual. In one case, for instance, two tests alike consisted in asking questions. But in the abstract-verbal one the right answer had to be selected out of four words. Whereas in the perceptual test one object had to be picked out of several presented in a picture. All tests were applied to 243 boys and girls ranging from 9 to 13 years of age.

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[1] Dissertation, Cathol. Univ., America (1929).

[2] *Brit. J. Psychol.*, xvii, 1 (1926).

As for the main results, these fell into two sections. In one of these, employing straightforwardly equation (M), Chapter II, the search for an abstract-verbal group factor was led to a definite affirmation of such a factor in tests 1, 2, 3, and 4, but a negation in tests 5, 6, 7, and 8.

But with these results have somehow to be harmonized the correlations obtained in the other part of this research. Here the correlations were on the average as set forth in the following table:

	Abstract-verbal	Perceptual
Abstract-verbal . .	·34	·34
Perceptual . . .	·32	·31

In this table, obviously, any abstract-verbal group factor should tend to increase the correlation in the N.W. quarter, whilst any perceptual group factor should increase the correlation in the S.E. quarter. Actually nothing of the sort is found. Instead, all four quarters have nearly equal values.

One explanation has been found in the supposition that in actually doing the test the procedure is about equally abstract-verbal all round. In any case, the relation between abstract and verbal has still to be elucidated.

Indeed so far but little heed has been taken of the fine work of Herring or of Stockton [1] as long ago as 1921.

IV. REDUCTION OF SAMPLING ERRORS

From the preceding unsatisfactory situation the study of the verbal factor was four years afterwards raised by Stephenson [2] to a height that in some respects has never since been surpassed, or even rivalled. The number of subjects rose to 1037, whereby the size of the sampling error was greatly reduced. The effect was like that of lifting a general veil over the whole picture.

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[1] Cf. p. 76.

[2] *J. Educ. Psychol.*, xxii (1931).

Hereby, in the first place, almost the whole factorizing technique could be further refined; for example, all the tetrads (see equation (C)) were carefully sorted into different types. Again, eight tests were employed of a carefully selected verbal kind, and as many more were non-verbal. Yet again, an abundance of irreproachable "reference values" were secured. Again, the subjects were very satisfactorily homogeneous both in age (10–12) and in other respects.

By these and other helps there was obtained a great number of fundamental conclusions, notably the following:

> "The non-verbal and verbal subtests have a high correlation, amounting to ·82 for a summed correlation for many subtests of both kinds. This fact stands in opposition to the opinions that have sometimes depicted the two abilities as independent.
>
> "In the case of the non-verbal subtests, the tetrad-differences matched with some exactness the value to be expected from sampling error."
>
> "In the case of the verbal subtests the tetrad-differences were appreciably larger than those to be expected from sampling alone. After making allowance for evident disturbances there remained excess error of amount approximately ·015, a value scarcely to be accepted as merely due to disturbances of the kind considered in explanation of it."

He found, furthermore, that

> "the evidence was against any group factor in the non-verbal subtests, but in favour of one group factor extending rather evenly through the verbal subtests."

On the whole, then, we appear to have here an experiment which lays strong claim to constitute, and even to locate what may be fairly designated as a verbal factor, "V".

V. REQUIREMENT OF "BREADTH"

For our present purposes, however, we need not merely factors but "broad" ones (see p. 79). By these we want to be able to make, if not affirmations, at least negations, that cover very wide grounds. Certainly, we must achieve results that include much more than just the more or less random eight tests considered above.

Now, as the earliest and perhaps still unrivalled study, extensive enough to secure the requisite breadth, a good claim can be advanced for the aforesaid Unitary Traits investigation. Here a large system of tests was devised, constructed, and indeed actually applied, of what were taken to constitute the essential classes of verbal process.

Doubtless other classifications could readily be substituted, and would perform the same office. And all such reasonable systems should reasonably well harmonize. Coming down to details, the classes here adopted began with the distinction between " comprehension " on the one side and " expression " on the other. Then the former was divided into Recognition of letters, that of words, understanding of words, and that of paragraphs or higher linguistic complexes. Turning to the other classes, those of Expression, here, too, five were adopted, namely, Choice of Words, Conformity with Grammar, Literary Composition, and the very dubious case of Generalization. In contrast with both divisions — and incidentally to pick from the vital work of Fildes, Gates, and Riley — an extra test was added of Recognition of Form. This consisted of small visual figures of exactly the same general kind as those used in the " Recognition of letters ", save only that they were *not* made to function as symbols of ideas.

Still more pregnant scientifically was a further plan of obtaining tests. Instead of simply picking out those which seemed to be essentially verbal, the procedure was to take cognitive operations so general that they could be effected *both* verbally *and* non-verbally. And so we are brought back to the old critical work of Davey. As affording such double employment were taken the following processes: eduction, reproduction, relation-finding, correlation-finding, perception, and even abstraction. Having thus obtained the tests and scores, the next thing was to factorize; this was in the main done as indicated by the theory of Two Factors (see Chapter II). In particular this treatment was that variety introduced by Holzinger under the name of "Bi-factor". All the ensuing loadings were given by him in his Preliminary Report on Spearman-Holzinger Unitary Trait Study, No. 9, 1936, p. 18, University of Chicago Publications.

Such was the original experiment, not only as planned, but even as executed. But plans "gang oft agley". On the study turning from the experimental over to the statistical side, there was a temptation for the original unitary whole to break up into numerous separated pieces. And thus, whereas the original comprised about 100 tests and 1100 subjects, even the biggest piece had only 49 tests and 355 subjects. To this fragment alone then we will here confine our exposition. A mere glance is enough to show that at least the requirement of "breadth" is satisfied. Let us go on to the more serious matter of agreement between statistics and psychology. Perhaps as illuminating an exposition as any will be to draw up a list of the tests showing this psychological factor and indicating what respective statistical loadings they bear.

VERBAL FACTOR

Psychological Description	Statistical Loadings
Word to complete sentence	·561
Understanding words	·533
Information	·522
Memory paragraphs	·451
Antonyms	·420
Understanding paragraphs	·384
Inferences	·338
Verbal analogies	·293
Grammar	·286
Composition	·250
Memory syllables	·198
Generalization	·122

At the top of the list, it will be noticed, come the two tests which deal with the simple employment or understanding of isolated words. Next, very significantly, comes "information". As for the rest, the loadings tend to become smaller as the operation gets more complex. For instance, as they bring in generalization, or introduce memory. But, on the whole, the fit between description and loading appears as good as could well be expected.

VI. RELATION OF "V" TO "INTELLIGENCE"

At this point we may conveniently take up the relation of the verbal factor to "intelligence". In ancient philosophy, as already mentioned, the start of the whole study seems to have been made in the concept of "logos". And this faculty was placed on an extremely high pedestal, mainly on account of its epistemological virtues.

But subsequently, on turning from the old-time faculties to our modern factors, it would seem to vanish, or, at any rate, to be shorn of its pristine dignity. As for the relation of V to G, these two, far from now being identified, are represented as mutually independent.

Here, however, a certain amount of statistical artificiality appears to intervene. In closer contact with actual observation is the relation of V to the current tests of what is known as Mental Age. Here the cited Traits study offers us the following values:

Test	Loading
Otis . . .	·430
Kuhlmann . .	·300
Morgan . . .	·336

Of even greater interest, however, would appear to be the relation of V to success in the various branches of study at school. The next table gives some remarkable results obtained by Alexander from 100 high-school boys having a mean age of about 17 years.

It will be noticed that to do well in English there is con-

Branch of Study	G Loading	V Loading
English	·259	·656
Science	·292	·477
Mathematics	·452	·351
Mechanical Drawing	·358	·173
Shop work	·243	·011

siderably more need of V than of G. What is more surprising still, the same relation holds good, only in somewhat smaller degree, for science. For here, too, success in school work involves more V than G.

Not until we pass on to mathematics does the said relation begin to be reversed.

Another and more recent work, that of Holzinger and Swineford, shows the following values for 145 children about 13 years old:

Branch of Study	G Loading	V Loading
Literature	·491	·474
Reading	·580	·468
Science	·606	·445
History	·525	·416
Language	·548	·302
Drawing	·360	·201
Citizenship	·298	·189
Music	·285	·181
Arithmetic	·562	·125

VII. STABILITY OF "V"

We have been recording the chief results hitherto obtained about the verbal factor in respect of its "breadth". There remains to be considered another characteristic of it; namely, what may be called its stability. In particular, how is it affected by the subjects' age and experience? On this difficult matter also some information may be afforded by the four groups of Alexander. We can derive from them the following respective loadings with the V-factor and — for comparison — with his F factor:[1]

Group	V	F
(1) 100 boys and girls, average about 12 years, elementary schools	·68	·35
(2) 71 boys, average 16 years, high schools, academically trained	·69	·51
(3) 100 boys, average 17 years, technical schools, practically trained	·68	·22
(4) 100 delinquent women, over 16 years, below normal on Stanford-Binet Scale . .	·66	·44

When we now proceed to examine the loadings for V, we see that their dominant features consist in being large and, above all things, in being uniform. This latter characteristic, moreover, is all the more surprising in view of the fact that the groups of subjects were very unlike, varying from small, normal children to adult delinquent. Then, too, we find that the said dominant features completely vanish in the loadings for F (practical factor). Curiously enough, too, the features are no longer present when we pass on from all these results from the data of Alexander to some others obtained recently by Denison and the senior writer at Chesterfield.

CHESTERFIELD DATA

Group	G*	K	V
(1) 51 girls, 11-12 years	·22	·49	·30
(2) 23 Colliery lads.	. .	·54	·53
(3) 42 boys, 13-14 years	. .	·36	·28
(4) 65 boys and girls (mixed lot) . . .	·32	·28	·0
(5) 59 boys	. .	·30	·50
Averages . . .	·14	·49	·43

* Here G denotes loading of tests intended to measure the common factor.

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[1] In calculating these loadings use has been made of the terms "XX" and "XY" employed by Alexander and others to denote respectively the average correlation within a class of ability and between different classes

In the preceding table, the blanks stand for (small) negative quantities. But the latter are counted in when averaging the residual correlation before taking roots for loadings.

VIII. RESULTS FROM OTHER METHODS OF ANALYSIS

So far, our account of the verbal factor has not yet got beyond the results derived from the original method of Two Factors, except that Alexander's results were obtained by the Centroid method with rotation of axes.

But as we have already seen (Chapter III), at least three other methods have been authoritatively advanced, namely, those of Summation, Centroids, and Principal Components. And these, although indeed they bring about little change in the relative loadings of the first factor, still produce great upsets. Thus now all loadings other than those of the first factor are now exactly balanced between positive and negative values; and this despite the fact that most psychologists hold that in the case of mental abilities negative values are generally irrational.

Faced by this paradox, the factorial psychologists have sometimes had recourse to a brave manipulation of the concept of "negation", pleading that it can be reduced to that of "polarity". But this seems to be only a jump out of the frying-pan into the fire. For mental abilities do not seem to be bi-polar any more than they are negative.

This is clear from a comparison with the sphere of the feelings; where the idea of bi-polarity does appear legitimate enough.

Other authorities, wishing to escape mere conceptual jugglery, have turned instead to definite mathematics. They have availed themselves of the convenient fact that by virtue of axial rotation the required factorization can be effected in

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(see his page 65). Then we argue that the excess of XX over XY measures the residual correlation left when any common factor in X and Y has been eliminated. Furthermore, we take the root of this residual correlation to give on the average the required loading. Symbolized, the loading is equated to $\sqrt{XX-XY}$.

an infinity of different ways; in particular it can be made to harmonize as near as may be with the theory of Two Factors. This has been done excellently by Alexander, Slater, and many others. But little less effectively in certain cases (especially with large enough errors) can the reverse be obtained of *differing* from the Two Factor pattern. Pre-eminent in this direction has been a study of Thurstone, wherein even the general factor G entirely vanishes. As for our verbal factor, this is replaced in this special case by *two* of them. One of these is described as being evidently characterized by its reference to "ideas and the meanings of words". The second Verbal factor is distinguished, it is said, as being

> "concerned with tests in which the subject deals with single and isolated words."

As an example of the former type of test is given that of Reading. For the latter type the author suggests the test of Disarranged Words.

The climax was reached by R. Wright,[1] who submitted 456 10-year-old children to thirty-two separate tests of the Stanford-Binet Scale.

Throughout, the work appears to have been done under the inspiration of Thurstone. And in respect of method, she followed her leader with great fidelity.

> "Correlational matrix was factored by the 'centroid method.'" Then "each coordinate plane was rotated independently until a simple configuration of the test variables was over-determined. The graphical method used is essentially that described in Primary Mental Abilities (Thurstone)."

Nevertheless, her results differ from his *toto coelo*. As regards the general factor, whereas he had made it disappear altogether, she now writes as follows:

> "The third column represents the most interesting development of the study: the appearance of a common factor. It has no nearly vanishing entries and no negative projections."

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[1] *Psychometrika*, vol. iv (1939).

As regards the verbal factor also she reverses his verdict, in that she returns from two to a single one. This is attributed to

- Vocabulary
- Paper Cutting
- Dissected Sentences
- Comprehension
- Definitions
- Repeating 5 digits backwards
- Absurdities
- Interpretation of Fables.

All except Paper Cutting and Absurdities are just the same as had been obtained by the Two Factors method, and the former of these is difficult to regard as essentially " verbal " anyhow.

But now to conclude, the quest of the verbal factor encounters a second dissonant note. It is sounded by the authority of Thomas Moore and his school. One of the outstanding studies has been that of Monaghan[1] in 1935. His subjects were 304 boys and girls, with an average age of about 15 years. This time mental ability was submitted to a complete new classification, namely, into Cognition, Memory, and Attention. Each of these classes had its own " general factor ", and on the other hand all three had conjointly a single " super general factor ".

And as for all the tests hitherto regarded as typically " verbal ", these were more or less randomly scattered through the three classes. For example, the test of " Words " was assigned to the first class; that of " Language ", to the second; that of " Alphabet ", to the third.

Moreover, following closely the previous one (1936), and apparently having a very similar theoretical basis, came the colossal research of McManama.[2] By him, no less than 2540 subjects of both sexes underwent the following five tests: Completion, Definitions, Discrimination, Analogy, and Proverbs. He found that his tests " measured consistently common ability from year to year ". This ability he describes as "the power to interpret and appreciate relationships". But of group factors, verbal or otherwise, he would appear to make little if any mention.

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[1] *Cath. Univ. Amer. Stud. Psychol. and Psychiat.*, vol. iii (1935).

[2] *Ibid.* vol. iv (1936).

And so our whole account of the verbal factor ends upon an unsolved discord. Beyond all reasonable doubt both the voices for and against the factor are alike true so far as they respectively go. The real question is only as to which and when one is more serviceable than the other.

IX. SUMMARY

The foregoing chapter has given an account of those factors which, although far from extending their respective ranges throughout mental ability of all kinds, are, nevertheless, apt to comprise at least large *groups* of them.

The general nature of the first and foremost of these "group factors" is indicated by their description as being "Verbal".

Naturally enough the topic had already — that is to say, before the advent of factors — received some more or less penetrating consideration. But this had been chiefly of a philosophical bent. Now, however, the inquiry took on a psychological direction. In particular, as set forth above, the study pursued the analysis of the process of reading, comparing this especially with sensory perception. But in time the further step was taken of introducing the aforesaid "group" factors. These had, indeed, been implicit already at the very beginning, 1904. But not till many years later do they appear to have received full formal recognition.

Their most essential characteristic (like that of the general factor itself) consists in correlation. All the ability comprised in any group factor should at least be wholly inter-correlated. In default of this virtue, the ability degenerates from a genuine factor to a mere faculty. In fact, the analysis must above all things aim at determining the definite points where the correlation begins and ends.

But such precise study — as, indeed, all exact statistical determination — is always obscured by a veil of sampling errors. Accordingly, our account has included some of the outstanding pains taken to reduce this veil to the least possible density.

But of far greater direct interest still have been the studies recorded which aimed at nothing less than a general survey of the whole length and breadth of the field to which the verbal factor rightly or wrongly lays claim. So wide, apparently, is this sphere, that the trouble is not so much what it includes as rather what it excludes.

Besides all such general direction, however, the study of the verbal factor has points of particular difficulty. One has been some encounter with the old flat, stale, and unprofitable equivocation of the term " intelligence ".

All too scant, on the contrary, has been the investigation of quantitative influences such as age, sex, heredity, and so forth. Most intriguing, and on the whole comforting, has been the comparison of the respective results of different methods of procedure.

And most hopeful of all, perhaps, has been the remarkable fashion in which the verbal factor has got to grips with the general fabric of scholastic, industrial, and even political life.

CHAPTER XII

MECHANICAL FACTOR

I. Original Conception of Spatial Factor
  (1) Non-Verbality. (2) "Performance". (3) Determination and Interpretation.
II. Complete Analysis
  (1) Procedure. (2) Total Correlations. (3) Residual Correlations. (4) Interpretation.
III. Reduced Findings
IV. Return to Complete Analysis
V. Stability
VI. Practical Applications
VII. Other Ways of Factorizing
VIII. Summary

I. ORIGINAL CONCEPTION OF SPATIAL FACTOR

(1) Non-Verbality. As seen already in the previous chapter, the earlier tests of "general intelligence" had encountered a grave set-back in their manifest dependence on some sort of verbal ability that did *not* appear to constitute anything reasonably entitled intelligence. This misfit was especially palpable when the said tests were applied to illiterate or to foreign subjects. For these immediately get into difficulties which obviously had nothing to do with the essential content of the tests, but only with their verbal expression. An outstanding instance occurred in the colossal testing of nearly the whole American Army during the First World War.

Consequently, whilst strenuous efforts were made to investigate this verbal influence directly, further endeavours were directed towards actually eliminating it. Several new tests were devised, grouped, or otherwise developed, by several leading psychologists — notably by Pintner,[1] in which the employment of words no longer appeared to play any appreciable part. One celebrated team of this non-verbal nature was used (particularly by the American Army) under the designation of " Beta ".

Unfortunately, this merely negative notion of " *non*-verbal " was soon found to be too indefinite for much scientific service. It soon dropped quite out of use.

(2) **" Performance "**. In its place were introduced tests designated as those of " Performance ". But this term, although perhaps at first sight more plausible, would seem on closer examination to be no less unclear. Surely the doing of *any* test can legitimately be called a performing of it!

If we turn from definitions to actual examples, we find such tests as the following: " X-O Series ", " Digit-Symbol ", " Number Checking ", " Cube Imitation ". Save that great pains seem to be taken to cut down to a minimum the use of language, there appears to be no rhyme or reason about the whole lot.

Certainly their interest does not lie in novelty. None of them has been more used than the spatial " forms " and " formboards ", but these go back to Seguin [2] in 1846; or even to Itard [3] in 1801.

Moreover, even if such terms as " non-verbal " or " performance " really did admit of precise definition, they would still remain embogged in the ancient morass of the so-called faculties; that is to say, they would make the unwarranted assumption of functional unity. For failing such an assumption, they would forfeit their usefulness; they would present something not susceptible of genuine measurement, either in the constitution of tests, or of individuals.

(3) **Determination and Interpretation.** Now in order to extricate the tests from all this embarrassment, and especially the last named, the sole effective resource is to pass from the ancient concept of faculties to the very modern one of

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[1] *A Scale of Performance Tests* (1917).

[2] *Traitement moral, hygiène et éducation des idiots* (1846).

[3] *De l'éducation d'un homme sauvage* (1801).

" factors ". The preceding chapter showed how this was done in the case of Language; the present chapter will have the same mission in the case of Space. Each case alike involves two distinct steps. The first is to determine a statistical coefficient or " loading ". The second step is to supply this determination with an interpretation.

II. COMPLETE ANALYSIS

(1) Procedure. We will commence our account with a study that may be credited with a privileged position; that of being the first in the field to attain anything like completeness. It is the work of J. W. Cox.[1] His subjects were 84 commerce students, 114 elementary schoolboys, and 228 trained mechanics. His tests numbered about 24. His method was that of Two Factors. In the following account we will confine ourselves to the largest group, that of the 228 mechanics. But the results for all the other groups were quite harmonious.

(2) Total Correlations.

	a	*b*	*c*	*d*	*e*	*f*	*g*
a Mechanical Explanation . .	..	·48	·47	·38	·38	·19	·52
b Mechanical Models	·48	..	·44	·40	·17	·29	·51
c Mental Efficiency II . . .	·47	·44	..	·43	·37	·35	·50
d Trade Knowledge	·38	·40	·43	..	·28	·15	·57
e Intelligence. .	·38	·17	·37	·28	..	·41	·54
f Mental Efficiency I	·19	·29	·35	·15	·41	..	·30
g Technical Tasks .	·52	·51	·50	·57	·54	·30	..

Comments. *a* and *b* are models; *c* has three sub-tests, paper-folding, 3-inch cube, jigsaw; *d* is " practical "; *e* is the senior author's verbal test; *f* is scholastic; *g* seems to be technical.

(3) Residual Correlations. Residues, left after deducting partial from total correlations, may be attributed to group factor " *m* "; they constitute its loading.

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[1] *Mechanical Aptitude* (1928).

	a	*b*	*c*	*d*	*e*	*f*	*g*	Loading with *m*
a	..	·39	·31	·29	·13	−·08	·34	·58
b	·39	..	·31	·32	−·14	·14	·39	·64
c	·31	31	..	·32	−·08	·00	·22	·49
d	·29	·32	·32	..	·07	·06	·40	·58
e	·13	−·14	−·08	·07	..	−·06	·12	·00
f	−·08	·14	·00	·06	−·06	..	−·10	·00
g	·34	·39	·22	·40	·12	−·10	..	·59

(4) Interpretation. There remains to carry out the psychological analysis of all seven tests (or other variables) in order to discover what mental character is present wherever *m* is, *and* absent whenever *m* is absent. As the tests increase in number, the required determination of the said character tends to become unique and absolute. Cox's own final solution is as follows:

> "*m* depends on the character of material and the kind of thinking. The former has to be 'spatial.' The latter involves not merely the apprehending of certain spatially arranged items, as in a design, nor solely in the eduction of their space relations. The ground of each new pattern lies in the immediately preceding pattern and must be educed therefrom by means of the spatial relations between certain of its parts."

He goes on to say that the same kind of thinking is clearly present in the Mental Efficiency II:

> "It is [he says] nearly always involved in handicraft, since this generally includes the planning of the shapes into which various parts of an object must be cut in order that when fitted together in a given way they shall make the required object. On the other hand, Mental Efficiency I involves no such spatial fundaments or relations. The eductive *m* work does *not* require either the nice distinctions in shapes and sizes or the executive skill needed in drawing and modelling. Again, *m*-tests differ from the 'practical ability' in which an object is actually constructed."

If we venture to condense this and much further fine description, the crucial character of the required "kind of thinking" consists in what the senior writer has called the educing of a correlate.

III. REDUCED FINDINGS

For contrast with the preceding work of Cox let us turn to two studies published in the following year by Earle with as collaborator in one case Milner,[1] and in the other case McCrae.[2] This time the numerous tests at issue were divided into five classes, as follows:

Firstly: "Those in which the apprehension of spatial relations appears to be an important factor in success." This group included the Formboard, Cube Construction, Immediate Visual Memory, Form Relations, and Mechanical Ability (individual assembly).

Secondly: "Those in which the use of language is the most dominant feature. Such tests are the Stanford-Binet Intelligence test and the Group Intelligence tests."

Thirdly: Those which do *not* appear to be much influenced by either the ability to deal with spatial relations or the ability to use language easily. "Such are the Cube Imitation, Substitution, Picture Completion, and Maze tests."

Fourthly: Motor tests (speed and dexterity).

Fifthly: Mechanical tests (the Stenquist improved).

The procedure consisted in determining the correlations, both between the five different classes, as also between members of the same class. From all these "total" correlations the authors proceeded to eliminate G, and so arrived at the "partial" or residual correlations. From these, again, the step was immediate to the loadings of the group factor. So far the procedure was just the same as that of Cox. It was simply that of Two Factors. But now came the crucial step of mental analysis or introspection. Here too Earle and his partners did outstanding work. They arrived at the following conclusion:

> "The special abilities entering into the performance tests under consideration appear to be unrelated except in

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[1] *The Use of Performance Tests of Intelligence in Vocational Guidance*, Indust. Fatigue Research Board, Report No. 53 (1929).

[2] *Tests of Mechanical Ability* (1929).

> the case of those in which spatial elements enter. . . . These are related by a rather small group factor for spatial perception as well as by the general factor G."

But surely this is not enough. Vainly should we try to mark off the field of the required group factor if we had no better criterion than that of being

> "a rather small group factor . . . in which spatial elements enter."

Very different was the definite analysis of Cox into, firstly, spatial relation finding, and, secondly, correlate finding. And, too, it undeniably furnishes a factor of great breadth. However, even less conclusive appeared to be the approach attained to anything of the sort in researches subsequently. Here may be quoted the blunt report of Line [1] that throughout his tests, indubitably "spatial" though they were,

> "there seems to be no evidence of a group factor."

What did make its appearance, in his "final experiment", at any rate, was an extremely high correlation with G. This, after making due allowances, amounted to no less than ·89.

So too, almost at the same time, Fortes [2] told us that

> "the tetrad criterion (Two Factors) proved to measure only G and specific factors."

A little later again Stephenson [3] — despite his extremely numerous subjects — arrived at the conclusion that

> "the evidence was *against* any group factor in the non-verbal subtests."

Subsequently, the same investigator, this time in collaboration with W. Brown,[4] finds only

> "some slight signs of a group factor, on the border of significance."

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[1] "The Growth of Visual Perception in Children", *Brit. J. Psychol. Mon. Suppl.*, xv, 91 (1931).

[2] "A New Application of the Theory of Noegenesis to the Problems of Mental Testing", Ph.D. Thesis, Univ. of London, 1930; cf. also *Trans. Roy. Soc. of South Africa*, vol. xx (1932).

[3] *J. Educ. Psychol.*, vol. xxii (1931).

[4] *Brit. J. Psychol.*, xxiii, 360 (1933).

Here we may also place the great "Traits" study mentioned above,[1] where the number of tests introduced came to nearly a hundred. Out of all these quite a large proportion could, in one way or another, be characterized as spatial. Thus several were said to be:

> "dependent on purely non-language . . . ability to perceive likenesses and differences between figures made up of lines, dots, circles, etc."

But of all that number no more than *three* tests were found to display any appreciable spatial group factor. Even of these three, two were only a little over the border-line of significance, namely, Test 2 (Perception II) with a loading of ·115 and Test 8 (Perceptual Analogies) with one of ·243. Still, the third remaining test, Nos. 3-4 (Perception III), that of rotating lozenges, showed on the contrary the high value of ·625.

Turning to Holzinger's two later works (in collaboration with Swineford), we encounter similar results. The number of tests displaying the sought group factor were respectively four, five, and six. The next table gives the loadings for all four studies; the first of them was called "E"; the other three were designated as "B". The G, of course, was the general factor for comparison.

And so we are brought to the end of a series of studies which, in general, may claim to rank topmost in the factorial analysis of the present day. What information have they after all supplied for the problem here at issue? The chief finding would seem to have been the presentation of certain factors as being characteristically "spatial", whereas others make no such claims. But little, if any, ground appears to have been won for making the required definite distinction between the one and the other. Nor is the situation substantially improved in this respect by adding the further undefined factors called E, B_1, B_2, and B_3.

So long as even this most fundamental requirement, a general criterion, is still lacking, we need scarcely be surprised

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[1] *Prelim. Report on Spearman-Holzinger Unitary Trait Study*, No. 9, p. 18.

Spatial Group Factor

TRAITS STUDY[1]

Test Nos.	Tests	Loads	
		G	E
1	Perception II	·810	·115
3-4	Perception III	·729	·625
8	Perceptual Analogies	·644	·243

PASTEUR STUDY[2]

Test Nos.	Tests	Loads	
		G	B1
1	Perception III	·585	·400
2	Cubes	·251	·433
3	Formboard	·224	·496
4	Lozenges	·450	·437

GRANT-WHITE STUDY[3]

Test Nos.	Tests	Loads	
		G	B2
1	Perception III	·589	·325
2	Cubes	·357	·309
3	Formboard	·445	·222
4	Lozenges	·518	·554

NINTH GRADE STUDY[4]

Test Nos.	Tests	Loads	
		G	B3
1	Perception III	·547	·354
2	Cubes	·408	·430
3	Formboard	·292	·464
4	Lozenges	·499	·595
5	Flags	·455	·435
6	Designs	·494	·232

at meeting serious disturbances in detail. All the more welcome has been the contribution made recently to the topic by Alexander. Already in the previous chapter we have seen the influence of language adequately eliminated by his tests of "performance intelligence" under the label of "F". Here we may add mention of the special evidence afforded by his Group 4, as presented by his table, which follows.[5]

Tests	Percentage Loading of			Chance Errors
	G	V	F	
Spearman's G	55	0	0	45
Verbal intelligence	40	41	0	19
Performance intelligence	33	0	25	42
Stanford-Binet	44	27	4	25
Reading-Thorndike	27	13	0	30

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[1] *Loc. cit.* p. 18.

[2] K. J. Holzinger and F. Swineford, *The Stability of Bi-Factor Solution*, p. 26 (1939).

[3] *Ibid.* p. 36.

[4] F. Swineford and K. J. Holzinger, *The Reliability of Bi-Factors . . .*, p. 14 (1942).

[5] W. P. Alexander, *Intelligence, Concrete and Abstract*, p. 97.

About this table he[1] formally announces that:

"This table is the proof of the existence of 'F.'"

Nevertheless, even here we are once more brought up against the same obstacle as before. The unknown criterion seems still to elude us. Granted — provisionally, at any rate — that it can be fairly taken to consist of "performance" or "practical" intelligence, we are still left asking what *this* is.

IV. RETURN TO COMPLETE ANALYSIS

In this predicament we need not be surprised to find a strong movement back to grapple once more with the problem as it had been conceived long before by Cox. Here the main business was not taken to be confined to calculating the statistical values of the factors, but regarded as at least as vital the interpreting of them.

Now along the changed direction the leader appears to have been Koussy,[2] under the general inspiration of Stephenson in 1935. The subjects were 162 boys, 11-13 years old, whilst the tests numbered 28. Of these latter some were visual, some visuo-spatial, the remainder miscellaneous. On this comprehensive basis there was again manifested a spatial group factor; it was called by Koussy "K", and was submitted by him to a treatment which surpassed anything effected previously, at any rate in respect of introspection. As an example of the result, the following is a remark made by one subject in the course of doing the test known as Pattern-Perception:[3]

> "I simply look at this shape (on the left) and when
> I look on the right I see it straightway."

In this case the introspection seemed to show that the test was essentially achieved by means of mental imagery.

In another case, however, the description given by one subject of the way in which he did the test of Classification was as follows:[4]

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[1] W. P. Alexander, *Intelligence, Concrete and Abstract*, p. 96.

[2] A. A. H. El Koussy, "Visual Perception of Space", *Brit. J. Psychol. Mon. Suppl.*, vol. xx (1935).

[3] *Ibid.* p. 82.

[4] *Ibid.* p. 83.

> "Success depended on a process of generalization and abstraction."

Here mental imagery did *not* appear to be essential. These contrasting results of the two kinds of visual spatial tests are set out by their author in the following table :[1]

Tests having significant K, and done by the aid of Imagery

Description	Load G	Load K
Memory of designs .	·66	·58
Form relations . .	·43	·49
Fitting shapes . .	·62	·61
Equations A . .	·67	·40
Pattern perception .	·76	·61
Spatial analogies . .	·63	·50
Band completion .	·65	·46
Correl. Eduction A .	·50	·62

Tests *not* having significant K, and done without aid of Imagery

Description	Load G	Load K
Classification. Spearman	·76	·22
Area discrimination .	·55	·00
Equations B . .	·54	·17
Equations C . .	·61	·25
Overlapp. Shapes. Stephenson . .	·67	·00
Overlapp. Shapes. Dir.	·64	·00
Classification. Spat. .	·49	·16
Correl. Eduction B .	·65	·01
Mechanical explan. .	·73	·09
Mechanical completion .	·55	·26
Woodwork . . .	·51	·20
Drawing . . .	·40	·19

As main conclusion the author writes :

> "There is no evidence for a group factor running through the *whole field of spatial perception*. . . . Spatial tests are primarily tests of G. But some spatial tests involve a group factor over and above their G content. This group factor called the K factor receives a ready psychological explanation in terms of visual imagery."
>
> "The explanation for the K factor consists in the ability to obtain and the facility to utilize visual spatial imagery."

In general form, surely, this study would appear to be unsurpassed. For here at last we come upon the double principle of Agreement and Difference in its complete development. To every test having the spatial group factor at issue, there duly belongs the appropriate imagery ; and analogously, in every case of the said group factor *not* presenting itself, so too the imagery fails to make its appearance. Interesting further is the harmony of the present work with the much older one of Cox. Both alike are characterized by the remarkable double basis; firstly, there is the mental content or material at issue; and, secondly, there is the way of thinking it. As regards the latter, the older view had the advantage of superior definiteness, in that it employed the unequivocal concept of "correlate educing", whereas the newer had to have recourse to the more popular and obscure notion of "utilizing". Passing on to the material or content of the thought, here the divergence seems to be more acute. With the older view, the material seems to have only consisted of visual movement. But with the newer work it consists of images.

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[1] *Ibid.* p. 77.

Precisely this point of Koussy's images it is, that chiefly attracted subsequently such outstanding investigators as Clarke,[1] Price,[2] and Slater.[3] There was a general tendency to concede that such things did play a part in the factorial analysis, but to deny that they possessed the supposed dominant influence. To the present writers, however, there seems to be room for a compromise. Really the dispute appears to be not so much a matter of fact as of words. Some psychologists confine the term "visual images" to the more stable, definite, and even coloured sort; whereas others extend the term to the more ghostly, fugitive, and faint. Possibly, then, the two ways of thinking at issue, those which do and those which do not favour the generation of spatial group factors, can be most significantly characterized as "concrete" and "discursive" respectively. To the preceding general account of the attempted psychological interpretations of factors may here be added a few words on the special case of such interpretation in connection with memory.

This is a field which has been elucidated by the work of Slater.[4] He set out with the following typical instance:

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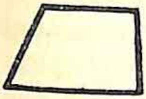
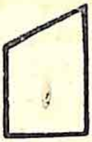
[1] G. Clarke, "Range and Nature of Factors", Ph.D. Thesis, Univ. of London, 1936.

[2] E. J. J. Price, "The Nature of the Practical Factor (F)", *Brit. J. Psychol.*, vol. xxx (1940).

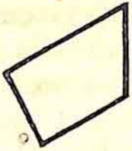
[3] "Some Group Tests of Spatial Judgment or Practical Ability", *Occup. Psychol.*, vol. xiv (1940).

[4] *Ibid.* xiv, 45.

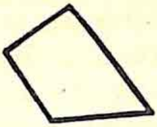




1



2



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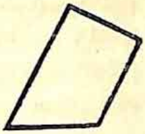

4

Here the question is " Which of the four given numbered shapes becomes like the unnumbered shape when it is doubly turned, firstly turned over like a penny from head to tail, and secondly round like a revolving wheel? " Now he found that in making this double turn two quite distinct mental processes are involved: first, the perceiving which of the four shapes is the same as the original one (process of recognition); and secondly, the perceiving which is in the right position (process of imaginative manipulation). He then proceeded to isolate each of these two processes in turn; he writes as follows:

> " In the *recognitive* problem, all four alternatives are in the correct position, but only one has the correct shape. In the *imaginative* problem, all four alternatives have the correct shape, but only one has the correct position."

The result was to manifest K *equally in both the manipulation and the bare recognition*; so it must, he said, be something more elementary than either. For this he suggests the name of " spatial judgment ".

There is one more point that can hardly be allowed to escape notice even in this brief review. The search for a spatial group factor has in one respect been over-successful. It has been found, not only once, but many times. Following up *m* there have been also K, F, E, B_1, B_2, and B_3. Are all these the same or different? What kind of evidence is needed to reach a decision?

V. STABILITY

By such consideration of the complete analysis of factors, we are led over to the question of their possessing other characters also which tend to render them scientifically stable.

First and foremost, among such requisites, come the character of unity. Now this has two general aspects, objective and subjective respectively. The former only concerns the statistical patterns and has been treated by us earlier in the chapter. The subjective unity, on the other hand, derives from the psychological nature of the factors. And if we accept the evidence of Koussy and others that this nature is comparatively simple, we seem forced to conclude that correspondingly all the determined spatial group factors *m*, K, F, E, B_1, B_2, B_3 are really one and the same. Among further possible sources of instability may be put such influences as Age, Sex, Experience, and so forth. An extreme instance has come from the work of Slater. In the beginning, he applied to 89 apprentices five verbal tests; seven tests of his "Spatial Judgment"; and two of Lines, Parallel and Divided respectively. For the statistical analysis he employed what he designated as the "Spearman-Holzinger bi-factor technique". The results were quite orthodox: all the tests except the Lines showing a G of about ·50. In addition, all the verbal tests had a V of about the same amount. Furthermore, all the previously found K-tests showed a K, again of about the same magnitude. The Lines had a very small group factor, apparently of no appreciable significance. Again, everything accorded with well established expectation. The author[1] himself wrote:

> "the results confirm the previous evidence that spatial judgment (K) is an independent psychological function."

Next year he carried out yet a third experiment.[2] This time the same verbal tests were employed. But on the other hand, all the previously found K-tests were replaced by tests so devised as to have no group factors at all and so of course no K (the tests were respectively Substitution, Analogies, Series, and Classification). The subjects were 82 children, 10 to 11 years of age. The analysis was said to be of the "Spearman type". All results proved to be still quite normal.

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[1] "Some Group Tests of Spatial Judgment or Practical Ability", *Occup. Psychol.*, xiv, 40 (1940).

[2] "Tests for Selecting Secondary and Technical School Children", *Occup. Psychol.*, vol. xv (1941).

But next year the same author executed yet another experiment.[1] The tests were of three kinds, these being much the same as before. But the subjects fell into two classes, these being respectively 211, aged 11, and 161, aged 13. The analysis was done by the Thurstone method. This time the result was altogether unexpected. With both classes of subjects, K now disappears, leaving only G and V.

The author's explanation was that, at 12 years, and even at 13, the spatial group factor is not manifested because it is still "immature". But this suggestion may not be easy to reconcile with other results obtained previously: notably Alexander's Group 1, and even Cox's elementary schoolboys.

Happier, however, for the peace of mind of the factor-analyst, has been the very detailed constancy and reliability shown by Holzinger to characterize the results obtained by him from different schools of the same general types. Moreover, the senior writer has attempted to assay the reliability on more difficult lines. He has picked out a more or less random sample of twenty tests executed at different times by different experimenters. The measurement to be found was of the frequency with which any factor discovered by any one experimenter to be present proved also present with another experimenter; and vice versa. Agreement of this kind ensued in 21 out of a total of 23 cases. The only two discordant cases were taken from two very similar models of Cox. These sometimes showed *m*, but sometimes not.

Herewith we come upon what constitutes perhaps the most serious of all disturbances in factor-analysis. This is the fact that one and the same cognitive task can often be done in two or more different ways. Here, again, is a trouble that goes at least as far back as Cox (see his pp. 145-146). Later on it was clearly described by McFarlane.[2]

For our present purposes we may stress the special liability of this influence to generate a spatial group factor.

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[1] "The Development of Spatial Judgment and its Relation to Some Educational Problems", *Occup. Psychol.*, vol. xvii (1943).

[2] "A Study of Practical Ability", *Brit. J. Psychol. Mon. Suppl.*, No. 8 (1925).

To it, for instance, may plausibly be ascribed the larger group factor shown on page 119 by test " Correl. Eduction A " than by " Correl. Eduction B ". Some tendency in the same direction, if less strong, is manifested in the larger group factor of test " Equations A " than " Equations B " and " Equations C ".

Those and many other analogous cases suggest that the subjects are inclined to drop the concrete procedure and take refuge in abstractions when they get into difficulties. For the latter alternative has the great advantage of one point at a time. Not impossibly, we may find at least a partial explanation of the paradox of Slater.

VI. PRACTICAL APPLICATIONS

Before closing the present account, a word must be added on one special advantage to which it may lay claim. It is that of being unusually applicable to practical purposes. From the very beginning, this fact has been indelibly registered in the very letter chosen to represent the factor. For " *m* " is meant to suggest something " mechanical ". To keep up the suggestion, marks were obtained for 228 trained R.A.F. mechanics in " a passing out technical examination ", as also in a " practical and theoretical test in trade knowledge and ability ". These two validating marks showed themselves to have correlations with the *m*-tests amounting to ·40 and ·51; whereas their respective correlations with tests of " intelligence " were only ·21 and ·42.

Furthermore, estimates were obtained for " ingenuity " based on (1) woodwork and (2) technical drawing and school examinations. These estimates, got for 59 elementary schoolboys, showed an average correlation of ·42 with the experimental tests, but only of ·08 with the school marks.

On the whole then, the author [1] was evidently at pains to introduce tests of immediate practical interest (engineering).

Next year, that is, in 1929, several correlations were — all too briefly — cited by Earle in partnership with McCrae and

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[1] J. W. Cox, *Mechanical Aptitude* (1928).

Milner [1] as having been obtained between some "Performance Tests" on the one hand and "Trade Proficiency" on the other. The results were as follows:

Test	Trade	Measure of Proficiency	
		Instructor's Estimates	Passing Out Examinations
Maze	Fitter	·44	·61
Cube	Fitter	·42	·46
Substitution . . .	Electrician	·38	·61
Formboard . . .	Smith	·62	·56
Picture Completion .	Smith	·48	·32

More extensive and detailed was the report of Allen and Smith [2] of work done at the Central Technical College, Birmingham. In the course of this research, a large number of tests, including several of the sort used to measure *m*, were applied to 60 men in their third or fourth years of apprenticeship and about 21 years of age. To be compared with these test results, estimates were procured of what were called "apprentice abilities" and were taken to consist in "combinations of quantities required, comprising intelligence, mechanical aptitude, mechanical ability, and manual dexterity".

The issue was disappointing. The average per cent of accordance of the tests with the estimates was not far over 60. The top three tests were Nuts and Bolts, Cox's Mechanical Models, and Memory designs. The three from bottom upwards were Cube Construction, Modified Stenquist, Porteus Maze.

The low values were chiefly due to the tests being too simple for these subjects. Further, the situation was obscured by mixing up the estimates for mechanical ability with those for dexterity.

A second research [3] was made by the same authors on 48 16-year-old pupils. The tests seem to have been the same. But this time the validation was the headmaster's record of last term's work in mathematics, machine drawing, applied mechanics, woodwork, and metal work.

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[1] *Loc. cit.*

[2] *Selection of Skilled Apprentices for the Engineering Trades*, p. 9 (1931).

[3] *Ibid.* p. 15.

Tests	% of Agreement
Cox's D (mechanical diagrams)	74
Verbal intelligence	74
Cox's M (mechanical models)	72
Form relations	71
Cox's E (mechanical explanation) . .	68
Cube	68
Memory design	65
Variable adjustment	65
Tracing	62
Pursuit	61
Maze	59
Nuts and bolts	57
Cox's C (mechanical completion) . . .	56
Stenquist	54
Formboard	53

Note:

General rise of correlation.
Special rise for verbal intelligence.
General rise of " mechanicals ".
Drop of Nuts and Bolts.

> " When allowances were made for particular traits, the agreement between the test results and the school ranking is raised to 93 per cent for the Technical College apprentices and 92 per cent for the Handsworth Technical School boys."

Selection for vocation. The boys who do better in the tests than in the examinations are on the whole superior in engineering jobs, like them better, and are found more satisfactory by their employers. All this is reversed in the case of non-engineering jobs.

There followed in 1936 by Farmer and Chambers [1] a remarkable attempt to see how the value of the tests varies in different occupations. For this purpose were used no less than 2731 subjects, divided up into 14 groups as follows :

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[1] *The Prognostic Value of some Psychological Tests*, Indust Health Research Board, Report No. 74 (1936).

F1 and F2 : 167 highly skilled apprentices, engineering.
H1-H4 : 1489 skilled apprentices, engineering.
A and C : 147, aged 14 to 38 years, simple repetitive work.
B1 and B2 : 373 men training to standard of good chauffeurs.
K1 : 121 men training in manual trades.
K2 : as K1, but having already had some selection and training.
S_1 and S_2 : 335, aged 18 to 29 years, trained in visual and auditory signalling.

The tests employed are shown on the following table of correlations. Regarded as "*m*" tests were: Cube, Mechanical Aptitude; Mechanical Ability; Form Relations. Criteria were in every case measures of "practical" proficiency (not merely scholastic examination).

Number of Cases	Group	Mean Score	Verbal Intelligence (33)	Construction of Cube	Mechanical Aptitude (Cox)	Mechanical Ability	Form Relations	Dial Test	Co-ordination	Dotting
	F1	129	32	00	..	..	..	..	24	00
	F2	122	47	32	..	..	..	..	00	00
339	H4	118	16	..	33	29	16	..	00	00
524	H1	117	12	..	..	..	..	..	19	12
347	H3	114	22	23	..	..	..	..	..	13
259	H2	105	11	26	..	..	..	..	..	00
	K2	99	00	..	..	..	00	..	..	..
248	S2	90	22	..	..	..	..	18	..	..
	S1	87	36	..	..	..	00	36	..	36
284	B2	86	41	25	..	..	41	..	..	..
	B1	84	32	45	..	39	25	..	55	24
121	K1	82	39	..	..	..	31	..	..	..
	C	76	00	..	..	..	..	..	00	32
	A	66	00	00	..	..	..	..	..	00

Note.—For N = 300, p.e. = ·04.

Unfortunately, this work "had the defects of its qualities". Its immense range had been bought at the fatal price of corresponding incompleteness and imperfection. Hardly anywhere is the material adequate for applying the standard formulae. Perhaps the most definite conclusion is the following :

> " The correlations between intelligence and proficiency are highest in those groups where the occupations make special demands upon the intelligence of those in it. This demand may be due to the inherent difficulty of the occupation as in F1 and F2, or because the occupation is difficult in relation to the intelligence level of those engaging in it as in the B's, K's and S's."

As for the required evidence on group factors, and in particular on the spatial group factor, this evidence is still inadequate. The situation seems especially complicated by the influence of speed preference.

The widely stirring work of Alexander[1] followed in 1935. His results relevant to the present theme were obtained with 103 boys whose mean age was about 17 years.

We may note that the loading with F actually surpasses that got for G in the case of " shopwork ", but is inferior on all the other counts.

Engineering and Other Ability	Loadings		
	G	V	F
Shopwork	·32	·01	·36
Mechanical drawing	·41	·20	·20
Mathematics	·56	·43	– ·02
Science	·34	·56	·06
English	·31	·79	– ·10

In 1937, Rodger,[2] too, studied the variations in the influence of *m* that arose from the character of the persons concerned. His subjects were 400 inmates of the Feltham Borstal Institution. These took up work in a number of different trades ; in each case they were divided into two classes, A and B, according to their being judged satisfactory or not. Furthermore, all these were submitted to several tests. The success of each test was assumed to be measured by the difference between its mean values for A and B respectively.

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[1] " Intelligence, Concrete and Abstract ", *Brit. J. Psychol. Mon. Suppl.*, No. 19 (1935).

[2] *A Borstal Experiment in Vocational Guidance*, Indust. Health Research Board, Report No. 78 (1937).

In the results, the trades fell into two well-marked groups. Group I consisted of the Fitters, Plumbers, and Woodworkers. Group II, Farmers, Labourers, Cooks, and Bakers.

Test	Difference for Group I	Difference for Group II
Intelligence	3·9	1·3
m-tests	3·9	1·5
Dexterity	1·0	1·1

Slater's study, which appeared in 1940, has already been quoted. His Table IV includes seven K-tests, one of discrimination of lines, and one of verbal ability. To confront these he has obtained two kinds of estimates; one of ability in "Engineering Drawing", and one in "General Apprentice Ability". The correlations proved to be as in the following table:

Test	Engineering Drawing	General Apprenticeship Ability
K-test (mean correlation) .	·41	·27
Lines test	·27	·28
Verbal intelligence . .	·06	·35

It will be noticed how the K-test has more affinity with the Engineering Drawing than with the General Ability; whereas the Verbal Intelligence has the reverse.

VII. OTHER WAYS OF FACTORIZING

All the way up to this point, our account of the factorial analysis has been restricted to one particular form of it; namely, that which has been produced from the original theory of Two Factors. But, as we have reiterated, an infinity of other forms are at least theoretically conceivable, and some few have even been actually attempted, notably that called the method of centroid. Still these few do not for the most part present any fundamental deviations. As regards the factors of the first order, these give values in just about the same relations, but only a little larger all round. And as regards the factors of higher orders, these differ more in mode of presentation than in substance. True enough they are usually made to undergo much more elaborate manipulation by rotation of the axes. But the Two Factors theory also has, if not the same necessity for manipulation, at any rate full amenability to be so handled. This theoretically expected similarity of the results to be obtained by factorization other than by the Two Factors theory has been confirmed by actual practice. For instance, Slater [1] gives us the following comparison:

TWO ANALYSES COMPARED

Thurstone 1st Factor	Spearman G	Thurstone 2nd Factor	Spearman V
·71	·74	·51	·48
·57	·57	·56	·49
·56	·56	·55	·61
·43	·40	·23	·24
·82	·83	−·01	..
·57	·52	−·08	..
·66	·65	·01	..
·55	·58	·22	..
·78	·70	−·24	..
·45	·48	·11	..

All this harmony, however, is liable to be rudely upset by introducing the much-contested principle of "maximizing the number of zero factor loadings". For this principle had been made to involve the sub-principle that each column of loadings should have at least as many zeros as it has reference vectors. Hereby, the theory of Two Factors is not indeed disproved but only arbitrarily denied.

Turning to the available more genuine evidence, this can hardly be found elsewhere than, as before, in the method of Agreement and Difference. This may here be otherwise expressed as minimizing the number of absurdities. And on this head, the factorizings we are considering cannot easily be exonerated.

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[1] "Tests for Selecting Secondary and Technical School Children", *Occup. Psychol.*, xv, 22 (1941).

The first and main work of Thurstone [1] himself, for instance, makes the tests presented as including, significantly, "the visual-spatial factor", include the quite incompatible one of "Verbal Classification (·411)", "Syllogisms (·430)", and "Sound-grouping (·412)". Or, take the employment of Thurstone's zero entries by Woodrow. Among the tests here showing the highest saturations with the visual-spatial factor is the extreme opposite of "Verbal Analysis".

We next turn to the important research by Drew.[2] This was undertaken at the suggestion of Alexander. The tests employed were three "Performance" tests, namely, Alexander's Passalong, Kohs's Block Design, and Cube Construction; the unpublished Chesterfield Group tests by the senior writer which are known as the GVK tests; and use was also made of the Moray House, Cattell, and Simplex tests together with some assessments.

There were 4 groups of subjects:

> Group A, 181 boys, age 11+; Group B, 172 boys, age 12+;
> Group C, 118 boys, age 13+; Group D, 88 boys, age 16+.

Thurstone's Centroid method yielded factors as shown in the following table:

Percentage of Total Variance

	Factor I	Factor II	Factor III	Factor IV	Factor V
Group A . .	33·2	10·1	6·1	2·9	..
Group B . .	31·1	10·6	6·5	2·7	..
Group C . .	28·0	8·6	5·0	4·2	2·8
Group D . .	37·6	11·7	6·2	4·25	2·7

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[1] *Primary Mental Abilities*, p. 79 (1938).

[2] "An Experimental Enquiry into the Methods of Selection for Technical Education based on Multiple Factor Analysis", M.Ed. Thesis, Univ. of Leeds, 1944.

After rotation of the axes in accordance with the Thurstone method there resulted the following table:

PERCENTAGE OF TOTAL VARIANCE

	G	V	F			Residual
Group A .	27·3	7·2	11·4			7·5
Group B .	28·2	10·9	6·7			6·1
	G	V	F	X	K	
Group C .	24·5	7·0	6·4	5·7	4·9	
Group D .	23·2	6·6	9·8	15·5	7·2	

The first or G axis was fixed through the Perceptual test of the senior writer. The second or V axis, at right angles to the first, was fixed through the Moray House test after G has been taken out. The third or F axis — F being Alexander's "practical" factor — was taken through the Passalong test. Drew further concluded that the fourth factor X was an achievement or character factor, and the fifth the K-factor.

Here are two of Drew's conclusions:

(1) F versus K:

> "Our results with Group C, in which we postulated a spatial factor, led us to rotate the axis until it passed through the Block Design Test, the residuum of which was held to contain a space factor. The loadings thus obtained lead us to believe that our hypothesis is confirmed. The factor is found in three performance tests, the spatial test and in the specifically technical subjects of the Diploma Examination. It is not present in English or the verbal Simplex test while it appears to play little part in Drawing. This result is supremely important. What it means is that the spatial factor is distinct from F, as our 13 + results led us to believe; it enters into technical ability as seen in the Diploma subjects, and, for the first time, the spatial test does, in some degree, measure K."

(2) Main Conclusion:

> "Technical ability is capable of being measured at the age of 13 and later by a performance scale comprising the Passalong, Block Design and Cube Construction Tests.

> The group test of spatial relations used in the research measures the K-factor at 16 but not at 13."

Note by the Junior Writer. When Drew's thesis had been completed a copy of it was sent to the senior writer, who was keenly interested in the work. He regarded this research as most important and devoted much attention to it. His comments were communicated to Dr. Alexander, to Mr. Drew, and to the junior writer. As these may prove useful to future workers in this field they are reproduced here:

Comments

(1) CRITICISM. In all the factorial analysis as nowadays cultivated, the fundamental problem is that of passing from some given set of correlational coefficients to some explanatory set of factors or loadings. But such a step is now universally taken to be possible in an infinity of different fashions. Consequently, the attainment of the required explanation must necessarily involve the introduction of some or other limiting conditions. Of these the present work makes use of three. The first and least questionable is that all the loadings should be positive, not in general negative. This condition we may pass over without demur. Very different is the second condition. This prescribes that as many loadings as possible should be zero. Moreover, this stipulation has been broken up into three distinct clauses (Thurstone, *Vectors of Mind*, p. 156). Of these some or all have been condemned as irrational.

More interesting and original, however, is the third limitation introduced into the present factorization. This consists, as we have seen, in passing the axes of reference through selected tests. Denoting the axes by the letters G, V, and F, these are rotated until they respectively pass through the tests entitled " Perceptual ", " Verbal ", and " Performance ".

Now here the whole argument hinges on the assumption that the rotation

> " converts the mathematical loadings into new and equivalent ones " [p. 88].

But to this assumption objections may be raised. The new loadings may be charged with not being " equivalent " wholly; with being so only in respect of their service of indicating points and angles in co-ordinate geometry; not in that of determining traits of individuals. More generally speaking, they may be said to apply solely to magnitudes that can be taken separately, not to those which depend essentially on being taken conjointly.

An example of the fallacy that may be introduced in this manner is the conclusion reached that

> " the loading of F in spatial tests is practically zero."

For this mathematical determination of zero appears to have been accepted by its author as characteristic of F essentially, or, at any rate, throughout all " spatial tests ". Whereas really it seems to have been only an accident befalling F in the very special and arbitrary case of the chosen rotation. More vital still is the objection to the device of rotation that this can only carry on the argument from one system of statistical values to another system of them. And so they afford little, if any, transition from statistics over to psychology; they afford no bridge, either from correlations to meanings, or vice versa.

(2) A Constructive Solution. In this difficulty of obtaining satisfaction by any or all of the three limiting procedures set forth in the foregoing work, we may the more readily turn to try out further feasible seeming solutions. Accordingly, we have had recourse to the common situation where the tests at issue can be arrayed in groups. An instance has already been supplied for us above, inasmuch as the tests there were groups entitled " Perceptual ", " Verbal ", and " Performance ".

Availing ourselves of this situation, we have gone on to assume here that all the correlations fall into two general categories; those within one and the same group, and those which extend to two different groups; intra-group, and inter-group, as they may be called. Further, the natural assumption has been made that the former will tend to be larger than the latter, the excess being usually ascribable to the group factors responsible for the groups. But the group factors are statistical entities, whereas the groups have a psychological nature. And in this fashion, the two different orders of being do appear to be brought together. Getting down to brass tacks, the following concept has been formed and employed as coefficient of Excess.

$$E_{xy} = \tfrac{1}{2}\,(r_{xx} + r_{yy}) - r_{xy},$$

the r's stand for averages.

Let us apply this formula to the A and the D groups of subjects. The results turn out to be as follows:

	E_{fv}	E_{fk}	E_{vk}
A group, 11 + years	·177	·027	·154
D group, 16 + years	·218	·040	·270

More insightful still are the following direct expressions of the respective partial or residual correlations due to the said group factors:

	$r_{kk \cdot g}$	$r_{ff \cdot g}$	$r_{vv \cdot g}$
A group, 11 + years	·004	·050	·304
D group, 16 + years	·103	− ·025	·462

With these results obtained by the method of Excesses, we may compare those got from the same experimental data by the above-described method of rotation. Of the ensuing differences of the results, a portion deals only with the calculated statistical factors. And in this respect we may chiefly note that the method of Excesses presents smaller magnitudes than does that of Rotation for the factors F and K, but little if at all for the factor V.

Still more important are the differences shown between the two methods in respect of psychological significance. For here, as indicated above, the method of Excesses claims to have sole validity. In particular the method of Rotation is found to have broken down altogether in the analysis of spatial cognition.

SUMMARY

To sum up the present chapter, the first and chief discovery has been that such a factor does exist and has been quantitatively determined.

Its nature, besides that of being spatial, has shown itself to have two primary characters. In the first place, the factor is concrete, not abstract. In the second place, its form is of that special kind that has been designated as " correlate eduction ".

Owing to this general nature of the factor, it possesses the cardinal requisite of " breadth ". In particular, it ranges far beyond the scope of the more or less artificial mental tests. Its function permeates also all engineering and allied scientific and industrial activity.

On the other hand, especially as compared with the analogous verbal factor, it has proved itself to be highly sensitive to disturbance. This character has been most evident in the case of young children; on this account, there would seem to be urgent need of yet further research.

CHAPTER XIII

OTHER BROAD QUALITATIVE FACTORS

I. Further Cases
II. Sensory Ability
III. Motor Ability
IV. Memory
V. Imagination
VI. Mathematics
VII. Psychological Ability
VIII. Summary

I. FURTHER CASES

In the three preceding chapters, which were devoted to a review of the recent progress of the analysis of "broad" factors additional to the general one, we have specially considered three cases; these may be respectively characterized as rational, verbal, and mechanical. But any number of others are, to say the least of it, conceivable. At any rate, we must look and see whether any such exist or not. As before, many important possibilities would seem to derive from the aforesaid noegenetic theory. Numerous further cases requiring at least investigation may be suggested along the lines of the old and new "faculties".

Indeed much actual research had already been recorded in 1927. And the positive as well as negative results, even where less precise or fundamental than those mentioned above, can, at any rate, boast the excellent merit of subsequent verification.

II. SENSORY ABILITY

One obvious region for trial is that of sense. Much that is commonly included in this category has been shown to fall more rightly into the all comprehensive domain of G. Another great slice of it has been annexed by the factor called mechanical. What remains for consideration? Is this merely an aggregate

K

of more or less independent elements — such as those presented by the sensory nerves — or does it present any important breadth of functional scope? An outstanding case is the question as to whether any functional unity binds together the different *senses* so that, for instance, any person excelling in discrimination of brightness will (irrespectively of G) tend to excel also in that of noises. This problem was the very first to need and to inspire the whole theory of factors. It was answered in the negative. Corroboration was afforded by Burt [1] as well as by Carey.[2] Concordant was the eventual interpretation of the results of Simpson.[3] Nor does subsequent work seem ever to have produced any serious challenge.

Even within one and the same sense there appears to be no comprehensive factor. Thus the discrimination of brightness has not been shown to possess any appreciable correlation (other than that due to G) with the discrimination of two points.

On the other hand, there actually enough does exist correlation between different sensory abilities involving any selfsame constituent. And even this linkage may directly or indirectly possess very wide and important scope. For instance, the mere power to discriminate pitch has been shown by Seashore,[4] Drake,[5] and others to dominate the entire sphere of musical talent.

III. MOTOR ABILITY

In many ways analogous to the ability of sense is that of movement, the resemblance between them being largely due to the fact of both depending much on the peripheral portions of the nervous system.

But, on the whole, motor abilities are even less unified functionally than the sensory. This much was already deduced from the early results of Perrin[1] (1921) and many others. Subsequently, it met with far more precise and careful demonstration from Earle and Cox. Even in the field of motor ability, however, correlations have not proved wholly lacking.

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[1] “Experimental Tests of General Intelligence”, *Brit. J. Psychol.*, iii, 177 (1909).

[2] “Factors in the Mental Processes of School Children”, *Brit. J. Psychol.*, viii, 79 (1915).

[3] C. Spearman, *Psychol. Rev.*, vol. xxi (1914).

[4] *The Psychology of Musical Talent* (1919).

[5] “Tests of Musical Talent”, Ph.D. Thesis, Univ. of London, 1931; cf. also “Factorial Analysis of Music Tests by the Spearman Tetrad-Difference Technique”, *J. Musicology*, vol. i (1939).

Long ago both Rao[2] and Reymert[3] announced it to link together various kinds of reaction-times. And in 1934 Cox[4] made the important discovery of a "routine manual" factor, which "appears to enter many occupations" and "should therefore find a wide application in the field of vocational psychology".

IV. MEMORY

Another interesting field, where one of the old faculties has given at least a suggestion for the quest after factors, is supplied by memory. Already by 1927 much research had been done in this direction, and, subsequently, it was vigorously continued. But upon analysis, it had readily broken up into more elementary constituents, of which the most characteristic was that of retentivity. To this heading, accordingly, our consideration of it may conveniently be referred.

V. IMAGINATION

Yet another and the last of the most important cases where the quality of one of the old faculties has furnished the basis for research after underlying factors is given by "imagination". But, unfortunately, this field, unlike the preceding one, is obscured by great equivocation.

In its oldest meaning, the term denoted the power of forming sensation-like "images". And here, unlike most cases submitted to scrutiny, a broad factor was immediately apparent. Between the abilities to form visual and auditory images respectively, a correlation was found by Carey [1] of no less than ·60. And this was in no degree due to G, for with this neither sort of image had any appreciable correlation.

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[1] "An Experimental Study of Motor Ability", *J. Exper. Psychol.*, vol. iv (1921).

[2] Ph.D. Thesis, Univ. of London, 1932.

[3] "The Personal Equation in Motor Capacities", *Scand. Scient. Rev.*, vol. ii (1923).

[4] *Manual Skill: its Organization and Development*, p. 237 (1934).

Another rendering of the term imagination, especially where coupled with the attribute "creative", is as what may be called invention. But here the noegenetic theory has taken a hand, by showing that the essential form of all mental creation or invention consists in educing correlates. And this process was shown by the senior writer, testing about 30,000 candidates for the British Civil Service, *not* to introduce any new factor, but instead only to constitute one of the two fundamental kinds of relational insight, as manifested in the universal factor G.

Less simple and more intriguing are the problems raised by a third kind of imagination; that which has been called "reproductive". The history of the case would seem to have begun in 1926 with Hargreaves,[2] who on using such tests as Inkblots, Free Completion, Unfinished Pictures, and Unfinished Stories, found in them a common factor (besides G) that though very small was remarkably firm and definite. It appeared to be of the nature of "fluency". Provisionally, however, it was given the non-committing label of "X".

The results of Hargreaves on "fluency" obtained corroboration in 1928 from the experiments of Karve,[3] who employed seven "Fluency" tests: Nouns, Unfinished Stories, Controlled Associations, Picture Completion, Prediction, Inkblots, and Free Associations. On eliminating G, Karve concluded that there was a group factor operative in each of the above tests of fluency.

The same year the junior writer applied the Nouns test, a Controlled Association test, and the Inkblots test to 23 cases of mania and 21 cases of melancholia. The results obtained bear out the general view as to the fluency of associations in mania and the lack of associations in melancholia.

A group of tests similar to those employed by Hargreaves was included by Holzinger and the senior writer in the Unitary Traits investigation. But, unfortunately, they were only carried through with such a small portion of the subjects that the said factor, if present at all, was masked by the comparatively large sampling error. About the same time, however, a factor of just this kind was strongly urged by two other authors, R. B. Cattell [1] and Studman.[2] The former identified it with a factor of " surgency " found as long ago as 1919 by Maxwell Garnett [3] and called by him C. Unaware of this simultaneous work, Studman for her part discovered just such a factor, now called " F ", to play a dominant role in mania (positively) and melancholia (negatively). For the moment, we need only note how all these discoveries, X, C, and F, turned out eventually to be always one and the same thing. The remarkable effects of this factor may be more appropriately studied in the field of character.

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[1] *Loc. cit.* p. 481.

[2] "The 'Faculty' of Imagination", *Brit. J. Psychol. Mon. Suppl.*, No. 10 (1927).

[3] "An Experimental Investigation of 'Fluency' in School Children", M.Ed. Thesis, Univ. of Leeds, 1929.

VI. MATHEMATICS

The factors we have been considering, although always very different from the old faculties, were, nevertheless, more or less derived from or suggested by these. But another such source of inspiration was supplied by scholastic needs and organization. An outstanding instance has been the endeavour to analyse " mathematical ability ".

First to make a fundamental approach to this problem was the very notable and suggestive work of W. Brown [4] in 1910. But to find a systematic application of the factorizing method we have to wait for Collar [5] in 1920, who, however, confined himself to arithmetic. Especially valuable among his results was the overthrow of the plausible doctrine that, over and above any general intelligence, arithmetic itself falls into *two* main subdivisions, a lower one for mere computation and a

[1] " Temperament Tests ", *Brit. J. Psychol.*, vol. xxiii (1933).

[2] *J. Mental Sci.* (1935).

[3] " General Ability, Cleverness and Purpose ", *Brit. J. Psychol.*, vol. ix (1919).

[4] " An Objective Study of Mathematical Intelligence ", *Biometrika*, vol. vii (1910).

[5] " A Statistical Survey of Arithmetical Ability ", *Brit. J. Psychol.*, vol. xi (1920).

higher one for problems, rules, and mental arithmetic. Really, the sole factor common to all the higher arithmetic does not stop short at this, but extends universally; it is, in fact, neither more nor less than pure G. The only thing that is genuinely characteristic of arithmetic lies in its lower or merely computative division. Here are the relevant correlations:

	Before eliminating G		After eliminating G	
	Computation$_a$	Higher Arithmetic$_a$	Computation$_a$	Higher Arithmetic$_a$
Computation$_b$	·85	·50	·74	·68
Higher Arithmetic$_b$	·50	·84	·68	·70
	Tetrad difference = ·454		Tetrad difference = ·058	

In good harmony with this conclusion have been the results obtained in 1918 by Rogers [1] and since submitted by the senior writer to the factorizing technique. The issue was to refute the common assumption of any factor (other than G) shared by Arithmetic and Geometry (pure). The union of these two into the single science as "mathematics" appears to be only a matter of practical convenience.

In agreement also are the results obtained by Fouracre [2] in 1924. On eliminating G, he found the correlations which are relevant to be insignificant, as shown in the following table:

	Secondary Schoolboys	Elementary Schoolboys
Arithmetic and Geometry . .	·14	·01
Algebra and Geometry . .	·05	·06

These fundamental conclusions and their comparatively early date have scarcely received adequate recognition. Most of the subsequent works on what has been called the "numerical" factor can hardly be regarded amendments or even an extension, but mainly as verifications.

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[1] *Experimental Tests of Mathematical Ability and their Prognostic Value*, Teach. Coll. Contrib. Educ., No. 89 (1918).

[2] "Psychological Tests of Mathematical Ability", M.Ed. Thesis, Univ. of Leeds, 1925; cf. also *The Forum of Eduction*, vol. iv (1926).

Among the most interesting have been those of Kelley[1] (1928), who arrived at the conclusion that arithmetical power involves a considerable amount of the verbal factor in the case of children of the third grade, but not of the seventh grade. In the Unitary Traits research, the independence between Arithmetic and Geometry is corroborated; but neither has any verbal factor. With Alexander, the verbal factor in arithmetic reappears. In the results of Swineford, it once more vanishes. Thus the remarkably variable content of verbality in arithmetic seems in need of further elucidation. In the Swineford[2] research we find much further verification of the close, but not exclusive, kinship of G with higher mathematics. Turning from all these results obtained about the numerical factor by the tetrad technique to those got by any method of factorizing, we find once more an excellent (though unmentioned) verification of Collar: the correlations of G with Addition, Subtraction, Multiplication, and Division have respectively the very high values of ·755, ·670, ·812, and ·619. The only other high correlation (·625) was with a test called Number Code,[3] a fact which would seem to support the view that the basis of the numerical factor is essentially a matter of forming associations.

VII. PSYCHOLOGICAL ABILITY

Finally, yet another ability claims our attention; but this time not so much because of the factorization which has been achieved of it, as rather that which is still conspicuously lacking. So far, we have been chronicling great labour and corresponding success in dealing with spatial cognition; and this supplies the basis of man's reaction towards his material environment. But we have been taught (notably by Strasheim[4]) that as much importance, if not more, attaches to his reaction towards his

[1] *Crossroads in the Mind of Man*, p. 136 (1928).
[2] Swineford and Holzinger, *op. cit.* p. 14 (1942).
[3] Thurstone, *Primary Mental Abilities*, p. 82 (1938).
[4] *A New Method of Mental Testing* (1926).

mental situation. And this ability, too, must needs have a cognitive basis. What is this, we want to know, and which broad factors, if any, are involved in it?

To a certain extent, the problem was already attacked by Webb in 1910. He invented two very different social situations, in each of which the choice of an advisable behaviour presented considerable difficulty. The result was to show correlation between the two cases that was far higher than could possibly be attributed solely to G; in other words, there existed here some or other group factor. Nor could this be derived in any degree from mere ability to handle the chief concepts characteristically involved in the planning of conduct, such as "wish" or "pleasure" as contrasted, say, with "heavy", "equal", or "absent". Here, then, was thought to lie some factor at least worth following up.

But this appears to have been done very inadequately. In the Unitary Traits research there was, indeed, one such test of skill in solving social problems, but it turned out to be quite unreliable. In the same research, there were also two tests of skill in interpreting expression and attitude respectively. Here, again, a group factor was indicated. But once more the technique proved to be so unsatisfactory that the results could only be taken as hints, not as proofs.

All this difficulty that besets the technique of testing skill in psychological interpretation recalls to mind the extraordinary fallibility proved by Magson[1] to characterize the judgments that men form of one another on the basis of personal encounter. As a means of diagnosing personality and, in particular, "general ability", "the interviews prove to be practically valueless".

VIII. SUMMARY

And so we complete our account of the factors introduced by the qualities of the mental processes at issue. The results, although less outstanding, both theoretically and practically, than those recorded in the preceding chapters, present still many notable features both positive and negative.

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[1] "How We Judge Intelligence", *Brit. J. Psychol. Mon. Suppl.*, vol. ix (1926).

Particularly intriguing is the result obtained that, save for mere computation, arithmetical ability has nothing in common save only G. How is this conclusion ever going to make its peace with the generally accepted observation that many people of high general intelligence have, nevertheless, low ability for mathematics?

Of much interest, too, is the further negation of any common factor characteristics of all, or nearly all, motor ability. As a general rule, it would seem, every kind of manual dexterity is independent of every other one. So far, there has only been found one considerable exception to this rule. On the positive side, a recent and possibly important discovery has been that of the factor designated as fluency.

CHAPTER XIV

SPEED

I. General Considerations
(1) Cognitive " Quantity ". (2) Duality.

II. Correlations of Parts
(1) Correlations of Parts with Parts. (2) Correlations of Parts with Wholes. (3) Isolation of Parts practically.

III. Experimental Disturbances
(1) Random Confusion. (2) Negative Factors. (3) Ability versus Preference.

IV. Bearings on Two-Factor Theory
(1) Vanishment of Group Factors. (2) The Great Paradox. (3) A Suggestion.

I. GENERAL CONSIDERATIONS

(1) Cognitive " Quantity ". If already the preceding factors offer no inconsiderable complications and difficulties, what shall be said of the one at which we now arrive of speed? Whereas the former were concerned with what may be called cognitive quality, this treats instead of cognitive quantity. Before, we had to cope with such processes as were involved in reasoning, in language, and in mechanical operations. But this time, our subject matter extends itself over all, or nearly all, cognitive processes whatever.

To solve this new problem, that of cognitive quantity, the temptation is apt to arise to proceed fundamentally; the attempt might be ventured to reduce all quantitative growth to terms that are really basic. As being of this nature we might turn to such terms as " clearness " or " clarification ".

In mental as in physical science, however, the characters most satisfactory for fundamental developments must usually, for practical purposes, be replaced by others that are more tractable. Thus the physical measurements purporting to measure "energy", "ether", and so forth, actually derive their content from visual percepts such as those of thermometers or galvanometers. In similar wise, the mental measurements are shy of grappling with the illusive "clearness" and have instead recourse to more or less indirect indications. Fortunately, this indirectness seems to do little harm for the practical purposes of science. Whether the measurements be direct or indirect, primary or secondary, essential or accidental, these are fine points by which the scientist, physical or psychological, need not often allow himself to be worried.

(2) **Duality.** Not so innocuous, however, is another obstacle in the way of measuring cognitive quantity. This consists in that the value obtained — whether direct or indirect or otherwise — is, at any rate, bi-valent. It possesses two dimensions. Besides speed in terms of simple time, it has accuracy irrespective of time.

Furthermore, the two dimensions are not, as happens in our previously considered cases, independent of one another. Instead they are intimately and complexly interdependent. And — what is worst of all — they are to a large extent arbitrary. Undeterred by all this has been the enterprising work of Howard,[1] who submitted a hundred children to several well-known tests of speed that seemed likely to throw light on the matter. His chief conclusion would appear to have been as follows:

> "The equation
> 'speed multiplied by accuracy = constant'
> is approximately true for each individual."

But at the same time he warned against understanding this in the sense of any strict proportionality. In one of his cancellation tests, for instance, he found that an increase in accuracy amounting to 2 per cent of its ordinary value of 98·7 entailed a decrease in speed no less than 10·8 per cent.

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[1] "Speed and Accuracy as Related to Intelligence and Perseverance", Ph.D. Thesis, Univ. of London, 1932.

II. CORRELATIONS OF PARTS

(1) Correlations of Parts with Parts. This bi-partite variable it was, then, that afforded some of the earliest topics of factorial analysis. And one of the earliest steps in this direction aimed at determining the correlation between different accuracies with each other.

Here among the most striking results would appear to have been got by Krueger [1] in partnership with the senior writer. Between accuracy of addition and discrimination of tone, they found a value of no less than ·69.

Another early result of Hart [2] with the senior writer was the correlation of the speed of addition with the following accuracies:

Sentences	Figures	Perception	Rings	Cancellation
+·40	+·42	+·10	+·09	+·14

(2) Correlations of Parts with Wholes. Akin have been such correlations as those (by the senior author) between, on the one hand, "G", and, on the other, the following tests:

	Accuracy	Speed	Ratio
Sentences . . .	·86	·64	1·34
Figures . . .	·81	·64	1·26
Addition . . .	·69	·50	1·38
Cancellation . .	·24	·35	0·66
Ticking rings . .	·23	·55 .	0·42

> "From these figures the surmise was made that with the top three tests the goodness was obtained under very different conditions from those holding with the bottom two. As regards the top three, it is achieved in the face of some intellectual difficulty; but with the bottom two the mistakes are of the nature of carelessness."

(3) Isolation of Parts practically. Supplementary — and in many ways more conclusive — was the evidence obtained by

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[1] *Zeit. f. Psychol.*, xliv, 81 (1906).

[2] *The Abilities of Man*, p. 249.

May under the direction of Terman. At two camps 510 men were given the American Army Alpha test; but then, using a differently coloured pencil, they were made to work further at it for as much time again. Despite this the two testings — short and long respectively — had an inter-correlation of no less than ·967.

This experiment was repeated by Ruch and Koerth at the University of Iowa. First, the ordinary test. Next, as above, a further equal period of time was allowed. Finally, the subjects were told to go on with it *as long as they liked.*

Yet the correlation of the first with the second was ·966, whilst that of the second with the third was ·945. Notable work was done by Bernstein, who used both "leisure" tests and "haste" tests and came to this conclusion:

> "No clear advantage can therefore be claimed by either the 'leisure' tests or the 'haste' tests." If anything, "an intermediate length of test seems to be the most desirable."[1]

III. EXPERIMENTAL DISTURBANCES

(1) Random Confusion. In general, however, the experiments of the preceding kind, in which the characters of accuracy and speed have been so far as possible mutually isolated, would appear in recent times to have been replaced by others in which the disturbances have been randomly thrown together.

In this way results have attained a bizarre nature. An instance is the following work of Whipple.[2] Here the class-standings of 50 boys were compared with their performances in the familiar Cancellation test: Here the correlation of the said standing with accuracy in the test proved to be zero, whilst with speed it was −·40. For this curious negative value diverse explanations may be suggested. But perhaps the most plausible one is that under such conditions the children lowest in the class are usually oldest and therefore usually cancel at greatest speed; this naturally generates negative correlations.

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[1] "Quickness and Intelligence", *Brit. J. Psychol. Mon. Suppl.*, vii, 49 (1924).

[2] *Manual of Mental and Physical Tests*, i, 319.

(2) Negative Factors. This brings us to a point of important technique. From the very beginning of factorial analysis, the assumption has been made that the factors at issue must necessarily be positive, but to this general rule, as we now see, there is one grave exception: it is that which arises from the need and yet fallibility of making allowances for speed. For here the negative sign is not only conceivable, but just as probable as the positive one.

(3) Ability versus Preference. Still greater trouble is caused by the fact that, whereas most influences are determined objectively, that of speed is almost arbitrary. Speed, like accuracy, becomes not a matter of ability, but only one of carefulness.

IV. BEARINGS ON TWO-FACTOR THEORY

(1) Vanishment of Group Factors. Turning from all such ways in which the influence of speed scores tends predominantly to generate experimental error, we may pass to its missions of supplying genuine information. And for this purpose we may here specially pick out the light thrown on the Two Factors. The standard procedure — as we have so often seen — is to pick out for the crucial tetrads two test-scores that do and two more that do not involve the characters at issue. Thus in the present case we can take any two that express accuracy and two more for representing speed. If neither pair show a correlation exceeding the limits of the required hierarchy, the presence of G is affirmed. Speaking more strictly, the tetrad is such that the members *can* be arranged hierarchically. Or, again, the hierarchy becomes " permissive ". And then alone all the correlations can be reduced to expressions of a single factor; all " group factors " can be eliminated.

Now typical of the results actually obtained by the authors mentioned above were the following:

	Figures (accuracy)	Figures (speed)
Addition (accuracy) . .	·61	·52
Addition (speed) . . .	·42	·26

Here the tetrad comes to

$$\cdot 61 \times \cdot 26 - \cdot 52 \times \cdot 42 = -\cdot 06,$$

which is negligible in comparison with the probable error. The same figures may equally be represented in the formula employed during the preceding chapters.

$$E = \tfrac{1}{2}(XX + YY) - \tfrac{1}{2}(XY + XY)$$
$$= \tfrac{1}{2}(\cdot 61 + \cdot 26) - \tfrac{1}{2}(\cdot 52 + \cdot 42) = -\cdot 035.$$

The conclusion is that — under the given condition — neither accuracy nor speed are linked into unity by any appreciable group factor.

(2) The Great Paradox. With this experimental conclusion, however, we would seem to be led up against a strange conflict with general experience elsewhere. For this does — if not explicitly, at least implicitly — accept speed as having a unitary nature in the sense that he who possesses it for one field of cognitive activity may, in general, be expected to have it in others also.

Sometimes this generality is taken in a depreciatory fashion. Thus Landor writes as follows:

> "Quickness is among the least of the mind's properties and belongs to her lowest estate. The mad often retain it; the liar has it; the cheat has it; we find it on the race-course and at the card table. Education does not give it and reflection takes away from it."

Still more frequently, however, this wonderful "property of the mind" is conceived in a complimentary sense. Through the ages the distinction has been drawn and stressed between the mind that can and that which cannot think quickly. When applying intelligence tests to senior officers at the Royal Naval College, Greenwich, the junior writer asked the Commandant, Admiral Sir William Pakenham, if he did not think it possible for a person to be very intelligent but slow. His answer was immediate and characteristic: " We do not want him in the Navy ".

(3) A Suggestion. Now one solution of the contradiction between the two disputants would be simply to side wholeheartedly with the one or else with the other. But the present authors have been unable to reconcile either of these views with all the available evidence. They have been obliged to replace both the extremes by a compromise. Instead of rejecting either of them, they would amend both. After all, the experimental evidence, on closer examination, only contradicts the existence of any group factor extending over the *whole sphere* of cognizing quickly. None of it is really opposed to the conception of such a factor covering the whole area, not indeed of speed, but of G.

CHAPTER XV

LAW OF INERTIA

I. PRECURSORS

II. EARLIER ESTABLISHMENT OF PERSEVERATION:
Wiersma, Heymans, Lankes, Wynn Jones, Bernstein

III. LATER CORROBORATION OF PERSEVERATION:
Pinard, Howard, Clarke, Maginess

IV. ADVERSE CRITICISM

V. INTERPRETATION OF p
(1) Problem. (2) Persistence. (3) Hindrance. (4) Recurrence.
(5) Inertia. (6) Rival Theories.

VI. FLUENCY

VII. SUMMARY
(1) Problem of Perseveration. (2) Results about Perseveration.
(3) Fluency. (4) Corroboration.

I. PRECURSORS

WE may now turn to a field which presents very striking similarities to, and also differences from, the abilities which we have considered. Like them it deals not only with mental quantity in general, but even with speed in particular. But unlike them, it is an outgrowth from the theory of types, a fact which has always dominated both its contents and its methods.

As regards the former, it has concerned itself not only with cognition, but even more so with volition and feeling: as regards its methods, these have tended to rely less on the formal procedures of modern factorization and more on various irregular indications.

Among the earliest attempts of this kind was the distinction portrayed by Plato between the "deep" and the "shallow" kinds of men, the former being characterized by "orderliness" and "constancy" whereas the latter are "carried haphazard". Among subsequent investigators of the same field, outstanding

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names are those of Aristotle, Malebranche, Wolf, and Beneke, whose works can still be read with profit. Passing on to the present century, however, we still find much the same types characterized under other names. Thus G. E. Müller [1] urges the importance of strong "perseveration" as conferring consistency of thought and action, but on the other hand prejudicial in vocations which need

> "a rapid disposal of numerous and quite different businesses."

Some years later, Gross,[2] Heymans, and Wiersma,[3] not unlike Plato of yore, arrived at distinguishing "deep-narrow" men from "broad-shallow" ones.

According to Gross,

> "each nervous element, whose (primary) functional excitement means the recurrence of a presentation in consciousness, persists for a further long period in a state of (secondary) after-function which remains regulative of the further direction of associative activity."

Accordingly, the deep type was designated a perseverative, the opposite or shallow type was — not very happily! — called "associative".

Even greater was the literary success — though not perhaps the scientific acumen — of Jung's distinction between the introverted and the extroverted types, of which the former are taken to turn their "libido" inwards, whereas the latter turn it outwards.

Such, then, in broad outline have been the general quantitative characteristics of cognitive ability in so far as these have been studied under the concept of "types" and designated by such terms as "deep" versus "shallow", "perseverative" versus "associative", "introvert" versus "extrovert". On the whole, the prospect offered in this fashion for the study of individual differences was brilliant enough but alarmingly insecure. These types had produced arrays of characteristics that were extraordinarily interesting if, and only if, they possessed functional unity; that is to say, if they were correlated together. And for their vital correlation there was for a long time little or no definite experimental evidence.

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[1] *Zeit. f. Psychol.*, Ergänzungsband, i (1900).

[2] *Die cerebrale Sekundarfunction* (1902).

[3] *Zeit. f. Psychol.* (1906–12).

II. EARLIER ESTABLISHMENT OF PERSEVERATION (*P*-FACTOR)

First in the field appears to have been Wiersma,[1] who in 1906 took the aforesaid " secondary function " to be exemplified by the lagging after-effects of sensory stimulation. One such effect was the number of revolutions of a colour-disc needed to produce fusion. Another case was the time needed to produce adaptation to darkness. A third consisted of the limen for an electric current after an intense one. Now all three tests were applied to 9 normal persons, 11 maniacs, and 18 melancholiacs. Every time, the melancholiacs showed far more of the secondary function than did the normal subjects, whereas the maniacs did the reverse. This result would seem to indicate that the function does act in a unitary manner.

In 1913 Heymans and Brugmans [2] applied a wider range of tests as follows: (1) fusion of colours as before; (2) light adaptation as before; (3) limen for flicker;[3] (4) limen for sound after loud noise; (5) pronunciation of difficult words; and (6) writing five letters as fast as possible 40 times in one direction and then 20 times in the reverse direction. And on this occasion, at least the device was introduced of correlation coefficients; and, subsequently, some formal factorization was attempted (see *The Abilities of Man*, p. 295). The results, so far as they went, were corroborative; the tests did show at least some tendency to inter-correlation over and above any effect attributable to G. But this result was inconclusive, since the subjects were only 15 in number.

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[1] *J. f. Psychol. u. Neur.*, vol. viii (1906).

[2] *Zeit. f. angew. Psychol.*, vii, 322 (1913).

[3] Cf. Ll. Wynn Jones, " Temperament and the Threshold for Flicker ", *Brit. J. Psychol.*, xxix, 422-426 (1939).

More seriously, then, must be regarded the research soon afterwards commenced by Lankes,[1] whose tests were nine in number and of widely diverse nature as shown below. They were applied to about 80 students in all. This time, however, a new difficulty arose. To secure the desired large number of tests, each had to be very brief, and was therefore too unreliable for separate consideration. Instead we can consider the whole set of results together. We may then note three points in particular: (*a*) practically all the correlations are positive; (*b*) they are about equally distributed among all the nine tests; (*c*) all the tests have been expressly picked to eliminate any influence of G.

From these three features conjointly, it is hard to escape the conclusion that the observed correlations are at least mainly due to some common factor over and above G. But for the purpose of dealing with such low and numerous correlations, there is commonly used another and more exact technique. This consists of merging the tests into one or more pools, such as averages or medians. This, as everyone knows, is a statistical device employed even in the very exemplar of ability measurements, that of " general intelligence ". So we may well pardon such an admittedly hard study as that of perseveration for seeking refuge in the same resource. Indeed we can only regret that the employment of averages in the present field of inquiry has not been more fully exploited. For, in the one case where it was done, the correlation of the said pool of perseveration tests with the subjects' estimated trait of perseveration was very important. The actual coefficient was -·26; or corrected for attenuation, -·40. The explanation offered (by the senior writer) was that, though perhaps a moderate amount of perseveration may be conducive to consistency of thought, feeling, and action, a very large amount of it may, on the contrary, exercise a disruptive effect. Anyway, the fact of this so diversely constituted and trivial-seeming set of tests correlating at all with any general trait of character might be taken to indicate that they are connected by some factor of considerable breadth.

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[1] *Brit. J. Psychol.*, vol. vii (1915).

We next turn to a research by the junior writer [1] which was carried out under the guidance of the senior writer. Of many tests devised, four were applied to a group of 77 children. They were the 2S test; the Backward Stroke Digits test; the Mirror Image Digits test (similar to the previous, except that in the second part of the test each digit employed was to be written as it would appear in a mirror instead of being written with a backward stroke); and the "it" test. These tests possessed a high reliability and as, moreover, they were group tests as distinct from individual tests, it was thought they would be likely to yield a rapid decision as to whether they possessed functional unity. Indeed, the tests showed distinct inter-correlation which was additional to what could be assigned to G and which seemed more reasonably explained by some further factor which might — without prejudice — be designated by the letter *p*. But this attempt suffered from the weakness, namely, that only four tests were employed with the major group and even these were of similar kind, namely, motor. Even so, they appear to measure a factor of wide extent. And it is well known that ever since most investigators on perseveration have depended largely on motor group tests.

Yet another outstanding research on perseveration was contributed during what we have called the earlier period of research. It was that of Bernstein [2] on 130 children. But here the tests were so numerous (ten) and therefore brief, that the inter-correlations were once more very low. Hence but little information could be expected from any of the tests separately. One fact noted, however, was that the mean tetrad difference had exactly the size most likely to arise from a single general factor.

Much more important were the following results obtained by throwing all the test-scores into a pool. This manifested a positive correlation of no less than ·51 with

> "long and careful observation made of the perseveration exhibited by the children in their ordinary school work;

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[1] *Perseveration*. Report of Brit. Assoc., p. 698 (1915); cf. *The Abilities of Man*, p. 295.

[2] "Quickness and Intelligence", *Brit. J. Psychol. Mon. Suppl.*, vol. vii (1924).

> in particular, note was taken of the differences displayed by them in the ease with which they started any fresh lesson."

If this result be accepted, surely it affords still further proof that the perseveration tests measure some single factor of great breadth.

In this argument, however, and in most of the work on perseveration there has been — as already mentioned — comparatively little utilization of the evidence supplied by the criterion tetrad difference. And this fact seems to have suggested to some writers that this criterion fails. For instance, Walker, Staines, and Kenna[1] rather energetically protest that in the case of the correlations of Bernstein, as also in their own research, a tetrad analysis yielded no general or even group factor. Unfortunately, however, these three authors do not appear to have really made any such analysis at all. In point of fact, all the said work — and particularly that of the three authors themselves — really yields as good a general perseveration factor as such material could possibly produce. The real criterion is a comparison of the mean observed tetrad differences with the theoretical value given by Formula 16A in the Appendix to *The Abilities of Man*. With Bernstein the two values were found to be respectively ·014 and ·013. In the work of the three authors the values are ·012 and ·011. Better fits are scarcely conceivable.

III. LATER CORROBORATION OF PERSEVERATION

Passing on from all this early work as summarized in *The Abilities of Man* (1927), the most remarkable further development has concerned the very peculiar connection found by Lankes between the tests of perseveration and estimates of mental stability. The suggestion had been made that stability is favoured, neither by low degrees of perseveration, nor still less by high degrees, but rather by medium ones. Now this comparatively early and unexpected result has subsequently received the most striking confirmation. For this we are indebted to Pinard.[1] At the beginning of his work, he tried to obtain ratings of children, character-traits according to a plan which he had preconceived. But the teachers objected that these were not the traits which they found to be really characteristic of individuals. Instead they proposed the following six: difficult, self-controlled, sociable, reserved, persevering, unreliable.

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[1] *Austral. J. Psychol. and Philos.*, vol. xix (1941).

But on calculating their correlations with the pool of perseveration tests, no positive values were obtained. But, next, he discarded the ordinary correlation coefficients and used instead a fourfold division of the children, according as these were found by the tests to be extremely perseverative, moderately so, moderately non-perseverative, or extremely non-perseverative. Here is the result:

> "On an average 75 per cent of the most difficult and unreliable children fall under the extreme forms of perseveration, while about 25 per cent group under the moderate forms of perseveration. The very opposite holds for the self-controlled and persevering children."

Pinard also carried out the same experiment on 116 patients at the Maudsley Hospital and the result was the same as that obtained with difficult children.

An experiment made later still by Howard [2] in a school for upper-class children gave similar results, but far less marked. The most "difficult" children again showed an appreciable tendency to be found among those who had very high p-values; but this time no such tendency was found among those who had very low ones.

G. Clarke's [3] investigation on children in Perth, Australia, also found that the difficult child was frequently an extreme perseverator or an extreme non-perseverator. A. Maginess,[4] a pupil of Stephenson, found that all the poorer qualities — poor intelligence, poor self-control, poor powers of concentration, poor physical energy, weak character in general — are associated with high *p*-score.

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[1] *Brit. J. Psychol.*, vol. xxiii (1932).

[2] Ph.D. Thesis, Univ. of London, 1932.

[3] *Some Character Traits of Delinquent and Normal Children in Terms of Perseveration Factor* (1934).

[4] M.A. Thesis, Univ. of London, 1936.

Another prolific investigator has been R. B. Cattell,[1] whose researches have not only corroborated those of earlier workers but have been extended to the realms of orexis and personality. He has recently [2] propounded the theory that " perseveration " as measured in a test is (D-W), where D is inherent disposition rigidity and W is the marshalled force of the ego — the will — trying to overcome this lag or rigidity. He believes that this is manifested most clearly in motor performances but may also be shown in switching of any kind. We are reminded here of a view of Lankes (page 163 of this volume).

IV. ADVERSE CRITICISM

Our account would be incomplete if no mention were made of investigators who have freely criticized our conclusions.

Jasper [3] cannot support the hypothesis of a broad group factor of perseveration, although he finds evidence for the existence of a narrow group factor of motor perseveration participating in a number of disparate motor processes, which require a more or less rapid shift from one pattern of response to another pattern within the same general type of response. Now a criticism of Jasper's work is that although he states that

> " many of his measures were near duplications of those used in previous studies,"

yet this does not appear to have been really the case. Of his fifteen tests, two were tests of simple reaction time, two were tests of choice reaction time, one was a test of star tracing in mirror vision. He included " Nouns " and " Towns " tests which would now be regarded as " fluency " and not as " perseveration " tests. He is apparently not aware of the fact that the Reverse Stroke Digits test as well as the " it " test were devised by the junior writer. Further, his questionnaire was surely more related to introversion than to perseveration. No wonder that he failed to obtain evidence of a broad group factor. It will hardly do to put all such tests together as if the score in each was mainly a measure of perseveration.

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[1] " Temperament Tests ", *Brit. J. Psychol.*, xxiv, 20 (1934). " Friends and Enemies ", *Charact. and Person.*, iii, 54 (1935). " The Measurement of Perseveration ", *Brit. J. Educ. Psychol.*, v, 76 (1935). " Perseveration and Personality ", *J. Ment. Sci.*, lxxxi, 151 (1935). " Temperament Tests in Clinical Practice ", *Brit. J. Med. Psychol.*, xvi, 43 (1936).

[2] " A Hypothesis in Answer to the Riddle of Perseveration and Disposition Rigidity ", *Charact. and Person.*, vol. xv (1946).

[3] *J. Soc. Psychol.*, vol. ii (1931).

We now turn to two papers by Walker, Staines, and Kenna.[1] Six motor tests were given in Australia to 205 university students. Unfortunately, the junior writer has been unable, up to the time of going to press, to ascertain the nature of these six tests. Apparently these authors do not rule out the concept of mental inertia as a factor running through all *p*-tests, but they are of opinion that other factors also are responsible for the data observed in *p*-tests, namely, disposition-rigidity, the degree of completion of the first activity, and the influence of the personality-traits of the subject.

These authors have also used a special method of scoring certain *p*-tests which merits consideration.

We are certainly in agreement with them when they express the need of a crucial investigation to ascertain the effects of such alleged factors as " disposition-rigidity ". We agree also in stressing the importance of ascertaining the best way of scoring each *p*-test.

V. INTERPRETATION OF *p*

(1) Problem. So far our point of view has been mainly statistical. We have been considering the evidence as to whether or not the tests expressly constructed by their authors to exemplify " perseveration " or " secondary function " have proved expressible in terms of one single broad factor called, provisionally, " *p* ". And the affirmative answer obtained to this question up to 1927 would appear to have been subsequently corroborated. The evidence for the statistical permissibility of such an analysis appears to have been unanimous. But there still remains, as in the case of G, the sister problem as to how this p can be envisaged psychologically. Here once more we encounter difficulty, and even controversy, but of less intense degree.

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[1] *Austral. J. Psychol. and Philos.*, vol. xix (1941); *Brit. J. Psychol.*, vol. xxxv (1945).

(2) **Persistence.** Now the earliest test results are those of Wiersma, whose express psychological significance, as we saw, was " secondary function ". This was described in more detail as the persistent effect of a presentation after it has quitted consciousness.

(3) **Hindrance.** Tests of similar significance were still retained by the other two Dutchmen, Heymans and Brugmans. But to them were soon added further tests which have since been described as manifesting

> " the degree of hindrance which the perseverating effect of past mental activity causes to a new one of the same kind."

For no given reason save convenience, these hindrance tests have almost always a motor character.

(4) **Recurrence.** A great further advance was achieved by Lankes. For his tests not only include those of persistence, and of hindrance, but, furthermore, harked back to the phenomenon of recurrence, as originally noted by Neisser, Pick, and Müller. But this great increase of scope has since been followed by a decrease. Lured perhaps by technical facility, most subsequent investigators have almost confined their tests to those of motor hindrance.

(5) **Inertia.** Up to this point, our description of the tests has been almost limited to bare observation. But it has left us with at least three different categories : persistence, hindrance, and recurrence. A near-lying enterprise is to trace back all three to some common constituent. If possible, it should be one already known, and therefore not invented *ad hoc*. Such a constituent trait, it has seemed to the senior writer, is afforded by the concept of " inertia ". Obviously enough, this covers both the persistence and the hindrance. The remaining

character of recurrence is not easy to explain by inertia alone, but still it seems readily enough reducible to a combination of this with another character which is anyway inescapable; it is that of "oscillation".

(6) Rival Theories. To conclude this account of the attempt to explain the phenomenon of perseveration by the concept of inertia, we must at least glance at one interesting endeavour to view the matter otherwise. It is that of Stephenson.[1]

He considers that at least four theories have to be kept in mind when searching for an explanation of *p*-factor measurements. The first is the theory of psycho-physiological perseveration, which he calls "continuance and hindrance effect". The second is one of fortuitous or extraneous mediation of "will"-like processes. The third is of intrinsic "will" function; that is, there are no fundamental individual differences, so far as *p*-tests are concerned, in continuance and hindrance effect but that the differences are explained by conscious or subconscious control, or lack of it. The fourth is that of mental "set", which completely inverts the usual theory of continuance and hindrance effect! It will be seen then that there is still ample scope for delving for explanations of *p*-factor measurements.

Stephenson's second and third theories both start from the fact that *p*-scores correlate (negatively and non-linearly) with estimates of character qualities. Lankes had concluded that the individual of high W could modify and change his own native tendencies to high perseveration and Stephenson regards this as tantamount to supposing that there are no native individual differences in perseveration. This view of Lankes, however, was not accepted by the present authors.[2] Neither can we subscribe to the view that the Spearman school regarded perseveration as a disturbing influence while other schools regarded it as a helping process. It may even turn out that different methods of marking *p*-tests would modify the results obtained by Pinard and others in relation to mental stability.

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[1] *Brit. J. Educ. Psychol.*, iv, 186 (1934).

[2] Cf. the senior author's view in *The Abilities of Man*, p. 302.

VI. FLUENCY

Besides perseveration, and all its manifestations, there has been found a second characteristic which is allied to, but not identical with, that of speed.

Its discovery appears to have been an accidental by-product of Hargreaves'[1] factorization of " inventive imagination ", using such tests as those of inkblots, free-completion, and unfinished stories.

In the main, this imagination showed itself to be simply resolvable into a correlate eduction combined with a mere reproduction. But over and above this there obstinately remained some admittedly " still rather obscure " specific correlation for further inquiry. This research of Hargreaves then singles out a fluency factor in the reproductive aspect of the sphere of imagination.

Corroboration of the results of Hargreaves was obtained by Karve in 1928. His tests being Nouns, Unfinished Stories, Controlled Associations, Picture Completion, Predictions, Ink Blots, and Free Association.[2]

A flood of light was thrown on it some years later by Studman's research [3] under pathological conditions. The factor underwent great increase in both statistical size and psychological clarity. Indeed we read now once more an almost ideal case of " agreement and difference ". Take such tests as those of " completion " or of " unfinished stories ". The factor enters into them just so long as they admit of numerous alternatives equally valid : a typical case is that of inkblots. On the other hand, the factor vanishes immediately the task is so framed that only one solution remains correct ; a typical case is afforded by the test of opposites. On the whole, the factor would appear to be most appropriately, if still imperfectly, designated as that of " fluency ".

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[1] *Brit. J. Psychol. Mon. Suppl.*, No. 10 (1927).

[2] Ll. Wynn Jones, *Theory and Practice of Psychol.*, p. 115 (1934).

[3] *J. Mental Sci.* (1935).

VII. SUMMARY

(1) Problem of Perseveration. In this chapter we have continued our record of the general quantitative individual differences in cognitive speed. But this task has led us up against an ability having several peculiar characteristics. First of all, its study has not sprung from any of the old " faculties " but rather from one or more of the comparatively modern " types ". Largely on this account, the concepts involved have been much more fluid and indefinite. First in the field was that of " perseveration " succeeded soon by that of " secondary function ", and this again by " introversion ". All three of these extended far beyond the region of cognition into that of volition and emotion.

But it was the original perseveration, and particularly its cognitive and motor sides that lent themselves most readily to experimental treatment.

(2) Results about Perseveration. As regards the results obtained, these showed themselves to suffer from unreliability. As in the case of measuring G, no single test, but a large pool of them, was imperatively needed. But given this, at least two valuable conclusions would appear to emerge.

One is, strong perseverators are hampered in switching over from one absorbing field of mental activity to another. The other is that *both* strong and weak perseverators are apt to act injuriously — though for different reasons — in self-control.

(3) Fluency. Another factor whose discovery has been chronicled is that of " fluency ". Unexpectedly enough this could *not* be identified merely with freedom from perseveration. On the contrary, the two trails appear to be independent.

(4) Corroboration. In general, all the results obtained in the older research have been subsequently confirmed and even developed. The sole exception would seem to be the extreme manifestations of primary and secondary function claimed to have been found by Heymans and Wiersma in mania and melancholia respectively.[1]

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[1] Ll. Wynn Jones, (1) *J. Ment. Sci.* (Oct. 1928); (2) *J. Nat. Inst. of Indust. Psychol.*, vol. iv (1929); (3) *Theory and Practice of Psychol.*, p. 199 (1934).

CHAPTER XVI

ATTENTION

I. The Concept of "Attention"
II. Experimental Evidence
III. Attention and G

I. THE CONCEPT OF "ATTENTION"

> "Not remote from the faculties of the Intellect and Intelligence is that of 'Attention.' Often this has been extolled as a discovery — indeed, the pre-eminent discovery — of quite recent times. But far from this really being so, it has a long and extraordinary past." [1]

The senior writer has reviewed this past [2] in detail and has surveyed the vast array of definitions of attention and conflicting views with regard to the term as exhibited in the writings of over sixty prominent authorities.

He has also shown that "Attention", instead of being correctly understood in the original sense of *directing the intellect,* has been taken to be anything else closely connected with this event.

> "Thus it comes to be defined sometimes as the clearness that *results* from such directing, at other times as the effort that *produces* it, or again as the bodily states that *accompany* it, and so on." [3]

In fact, seeing that "attending" and "being conscious" are so closely related, this is not surprising.

The concept of "Attention" would not be expected to receive much favourable consideration from behaviourists, but Gestalt psychologists also from their very different viewpoint have voiced their criticisms of the term. Thus Helson [1] writes that "Attention" is found in every imaginable sense

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[1] C. Spearman, *Psychology Down the Ages*, i, 133 (1937).

[2] *Ibid.* chap. vii.

[3] *Ibid.* p. 145.

> "as a function, an act, a condition, a state, an elementary constituent of perception or separate act in addition to the others, as clearness, vividness and so on *ad libitum*."

Koffka links it up with the constancy hypothesis to illustrate one of the worst uses to which it has been put from the configurational point of view. Thus when an object, sensation, or association surprises us by its appearance, or by some change which we have not expected, attention serves as an explanation.

Koffka's view was that the best configurations do not receive the maximal amount of attention, and configurations may receive a maximum of attention and yet remain amorphous, ill-structured aggregates. As an attempt to avoid this imbroglio may be regarded the introduction of the concept of "set" instead of "attention" in contemporary psychology. But apparently with questionable success. Gibson,[2] in an important review, has no difficulty in concluding that no common meaning can be discovered in the list of variants of the term as found in recent articles:

> "Mental set, motor set, neural set, voluntary set, unconscious set, postural set, organic set, preparatory set, task-set (Aufgabe), situation set, goal-set, temporary set, permanent set, set to react, set to perceive, expectation (expectancy), hypothesis, anticipation, foresight, intention (aim, end, purpose, determination), attitude, directing tendency, determining tendency, tension, vector, need, attention, perseveration, preoccupation!"

After a survey of experiments on reaction time, association, learning, perception, thinking, task completion, and conditioning in the effort to discover a common nucleus of meaning for the term *mental set* and its variants, Gibson found no common meaning but instead a number of ambiguities and contradictions. Small wonder, then, that the term "attention" is seldom found in the index of recent psychological text-books.

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[1] "Psychology of Gestalt", *Amer. J. Psychol.*, xxxvi, 356 (1925).

[2] "A Critical Review of the Concept of Set in Contemporary Experimental Psychol.", *Psychol. Review*, vol. xxxviii (1941).

In 1908, Titchener[1] wrote:

> "The system of psychology rests upon a three-fold foundation: the doctrine of sensation and image; the elementary doctrine of feeling, and the doctrine of attention." . . . "I think that the experimentalists may justly point to . . . the discovery of attention."

Such views could claim little support to-day.

Without in the least detracting from the importance of the many experiments on the accommodation, range, duration, or fluctuation of "attention" which were begun by Wundt as early as the 'sixties and continued up to the present time, it can at least be said that they have not proved the existence of a faculty of attention. Even Titchener had to admit that the alleged discovery of attention did not result in any immediate triumph of the experimental method. It was something like the discovery of a hornets' nest; the first touch brought out a whole swarm of insistent problems.

Titchener[2] regarded attention as a *state* of consciousness, but as already indicated there have been many other interpretations.[3] Thus Pillsbury[4] regarded it as a *process*, whilst Stout and later McDougall stressed its *conative* aspect.

> "Attention is simply conation so far as it requires for its satisfaction fuller cognisance of its object without other change in it."[5]
>
> "For 'attention' is merely conation or striving considered from the point of view of its effects on cognitive process."[6]

The trend in theories of attention, with special reference to recent American writers, is illustrated by Paschal,[7] who finally decides to keep the term attention after defining it in a sense which meets the condition set down by the senior writer, namely, that it refer to the direction of mental energy.

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[1] *The Psychology of Feeling and Attention.*

[2] *Experimental Psychology*, vol. i, pt. 1, p. 110 (1906).

[3] Cf. *Psychology Down the Ages*, chap. vii.

[4] *Attention*, 1908.

[5] Stout, *Manual of Psychology*, p. 125, 3rd Ed. (1913).

[6] McDougall, *Outline of Psychology*, p. 271, 2nd Ed. (1924).

[7] Paschal, "Trend in Theories of Attention", *Psychol. Review*, vol. xlviii (1941).

II. EXPERIMENTAL EVIDENCE

In 1916 Woodrow [1] carried out an investigation which he claimed

> "resulted in the experimental demonstration — probably the first such — of a general capacity for attention on the part of any individual."

But there were only 12 subjects: 7 adults, 4 boys from 12 to 15, and 1 boy of 8 years: of the adults, 4 had had previous work in reaction-times. It must also be pointed out that the four tests used were all reaction-time tests, namely, simple reaction to touch, to sound, to light, and choice reaction to an increase or decrease in the intensity of light. He writes:

> "One might attempt to identify the individual's general capacity for attention with the factor called 'general ability,' and to analyze all the other conditions of attention into complexes of very specific elements. In such case, the present study might be regarded merely as confirmation of Spearman's theory."

To this research we shall return in the next section of this chapter.

The next research to be considered is that of McQueen.[2] The plan was to perform a number of experiments involving distribution of the attention and to see if any evidence were forthcoming of the existence of any general factor of distribution of the attention. The tests employed were: Tapping and Adding, Card-sorting and Counting by 3's, Putting Discs on a Needle and Crossing out o's, Discrimination of the Size of Circles tachistoscopically exposed, and Dotting. McQueen concluded that a general power of distribution of attention does not exist but that the power of distribution seems to be specific in every case. Thus performance in the tachistoscopic test seemed to depend very largely on a specific factor, the *persisting image*. What happens when two disparate operations are simultaneously performed? McQueen produced introspective evidence from trained psychologists showing that all of the following four possibilities may occur:

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[1] "The Faculty of Attention", *J. Exper. Psychol.*, vol. i (1916).

[2] "The Distribution of Attention", *Brit. J. Psychol. Mon. Suppl.*, vol. v (1917).

(1) the attention alternates from one to the other;
(2) one of the tasks becomes automatic, and drops out of consciousness, occasionally even both;
(3) the processes combine into a psychical fusion, an easy form of fusion being through the medium of rhythm, though the rhythmization may be unconscious;
(4) there may be simultaneous attention to the two tasks as separate. This is apparently very rare. But where it does occur the conscious *effort* is directed to only one of the tasks, the other being performed *passively*.

This passivity seems to be incipient automatization. McQueen's results also support the supposition of the existence of a general factor G, and one manifestation of this general factor consists in the ability to concentrate the attention.

It remains to mention a number of recent researches. Easley's[1] attempt to isolate the factor of attention gave negative results.

Woodrow[2] studied the common factors in fifty-two mental tests, among which were tests specifically prepared for the measurement of attention, and found his fourth factor to be of a type he elected to denote " attention ".

Parry,[3] a pupil of Burt, studied the role of attention in aesthetic experience and found that 65 per cent of the variance is attributable to a general factor.

Wittenborn,[4] using tests designed to measure what was conceived to be attention, found by factorial analysis a factor which is independent of the factors of rote-memory, visual space, number, or perception. To a large extent, at least, this attention factor is independent of content and of mode of presentation of test material. The tests in which the variance is mainly dependent upon this factor are those involving a high degree of sustained or relatively continuous mental work.

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[1] *Amer. J. Psychol.*, vol. xliii (1931). [2] *Psychometrika*, vol. iv (1939).

[3] Ph.D. Thesis, Univ. of London, 1940.

[4] *Psychometrika*, vol. viii (1943) (not seen, reference to it found in *Psychol. Abstracts*, vol. xvii, 1943).

III. ATTENTION AND G

We have already referred to the speculative character of views as to the nature of " attention ", of " intelligence ", and of " mental energy ". It is therefore not surprising to find that various views have been put forward as to the relation of attention to G. Thus Woodrow[1] writes:

> " The identification of general ability, or the essential factor therein, with capacity for attention has often been attempted, but with questionable success. Spearman himself declines to admit such identification. Without doubt, the question of the identification of the general capacity for attention with general ability should be regarded as an open one."

When we examine the statements of authors which he lists as favourable to such identification, it can be seen that they must be regarded as speculations. Nevertheless, it is always profitable to consider very seriously the speculations of such preeminent authorities as Binet,[2] Wundt,[3] Burt,[4] or Garnett.[5]

It is well known that the various speculations of Binet as to the nature of intelligence were hardly consistent. As for Wundt, there are two sentences from his classical treatise which are often quoted. The first[6] consists of a rather unclear sentence consisting of as many as 117 words! The second[7] starts with a modest "It may be" (*Es mag also sein*). It is also clear now that Sir Cyril Burt's sentence,[1] which is often quoted,

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[1] *J. Exp. Psychol.*, i, 316 (1916).

[2] *Année psychol.*, vi, 393 and 395 (1899).

[3] *Grundzüge d. physiol. Psychol.*, i, 381; iii, 598 (1908).

[4] *Brit. J. Psychol.*, iii, 166 and 169 (1909).

[5] *Education and World Citizenship*, p. 117 (1921); cf. also *Knowledge and Character*, p. 145 (1940), by the same author.

[6] *Grundzüge d. physiol. Psychol.*, i, 381, 6th Ed. (1908).

[7] *Ibid.* iii, 598.

> "And in this sense we may agree that so-called 'voluntary' attention is, of all recognised psychological processes, the essential factor in General Intelligence,"

must be interpreted with caution. It was merely an attempt, on the basis of McDougall's view of the attention-process, to reconcile the views expressed by Wundt and Binet with those of other writers:

> "This suggested reconciliation, however, was merely a corollary to my conception of intelligence, not the primary definition of it. Indeed, instead of maintaining that 'intelligence is merely an effect of attention,' I would rather say that attention (so far as it is cognitive and not a conative phenomenon) is an effect or symptom of what we call intelligence."[2]

Garnett[3] takes G to measure "capacity to concentrate attention". Now one difficulty is to reconcile such a view with the results of a research by Koch and Habrich[4] on the ability to cognize *without* attention.

The conclusion would seem to be that:

> "Whether the operation is performed attentively or inattentively, with high or with low degree of consciousness, with the focus or with the fringe of the mental energy, with or without expressly directed effort — all this would seem to make no difference in the dependence of the cognition upon *g*."[5]

Indulging then in speculation we may define attention as "the application of intellectual energy". Such an interpretation of the word seems not inconsistent with the writings of Burt, of Woodrow, and perhaps even of Garnett.

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[1] *Brit. J. Psychol.*, iii, 169 (1909).
[2] C. Burt, *The Factors of the Mind*, p. 217 (1940).
[3] *Op. cit.* p. 355.
[4] *Zeit. f. ang. Psychol.*, vol. vii (1913).
[5] *The Abilities of Man*, p. 344.

CHAPTER XVII

RETENTIVITY AND MEMORY

I. Retentivity
II. Early Factorization
    (1) Memory. (2) Sensory Memory. (3) Verbal Memory. (4) Non-Verbal Symbolic Memory. (5) Formal Influences. (6) Upshot.
III. Later Memory Studies
IV. Bi-Factor Studies
    (1) Experimental Set-up. (2) Corroboration. (3) Inadequacy. (4) Land of Promise.

I. RETENTIVITY

As next in degree of universality we may well take the concept of " retentivity ". For, to some extent at any rate, this too appears to enter into cognitive operations of all sorts and descriptions. Unfortunately, however, the concept has rarely, if ever, been treated by current psychology in any adequate manner. There has not even been preserved the most fundamental of all its features, which consists in its essential distinction from everything that can reasonably be termed " intelligence " (or " noegenesis "). In place of any such sharp distinction between the two has been admitted the grossest confusion.

Realizing, however, the unprogressive nature of this condition of study, the factorial analysis soon betook itself to the quest after some more definite concept. And this was assumed to have been supplied in the idea of " Memory ". But even this concept, although indeed not so universal as the other one, is still extremely broad and confused.

II. EARLY FACTORIZATION

(1) Memory. In order to realize such a notion in experimental actual practice, it had to be taken to extend at least wide enough to include such diverse things, as on the one hand sensation, and on the other hand language. Then the sensation was in part represented by shades of yellow, sizes of angles, variety of pattern, and groups of colour. The other part of the sensory content to be memorized was made auditory and was represented by tones of pitch, rates of ticking of a metronome, and phrases of music. As for the further requisite, that of language, this was served by the tests of sentences and that of Association (connecting words with numbers and with letters). Now the question immediately at issue was as to whether or no such extremely wide limits, but excluding G, can be comprised in one single factor. The result was to indicate that such a broad factor does exist. But on the other hand, it would appear to be a very small one.[1] Thus the average specific correlation between the verbal and the visual memory was only ·11; that between the verbal and the auditory ·19. Such amounts are clearly insignificant. They do nothing to support the common practice of speaking of the memory as a single general ability. This comes most plainly to view when the attempt is made to set forth a person's mental "profile". The preceding observations would seem to suggest rather an undefined multitude of different abilities.

(2) **Sensory Memory.** Let us pass on to ability that is again broad, but this time distinctly less so. We may take the case where the abilities compared are indeed restricted by the qualification of alike being sensory, but on the other hand are not limited by being confined to one and the same sense. Thus Carey's work supplied several tests of both visual and auditory memory. The inquiry had to ask whether or no there was any group factor extending over from the one kind to the other. This time the reply was that such a factor was not only present but to the appreciable value of ·27 (with a p.e. of ·05). This positive result was all the more remarkable by its contrast to that of comparison between visual and auditory discrimination. For in *this* case such a group factor was shown *not* to be present.[2]

(3) **Verbal Memory.** Let us next turn to the problem as to

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[1] N. Carey, "Factors in the Mental Processes of School Children", *Brit. J. Psychol.*, viii, 80 (1915).

[2] Carey, *op. cit.*, p. 90.

the existence of a factor of verbal memory. In other words, is there a group factor running over all verbal memories however diverse in other respects. For a test example we may take the cases of memory for sentences on the one hand and that of memory of lists of disconnected words on the other. The answer appears to be in the affirmative. Thus Abelson[1] found such a correlation amounting to ·44.

Similar, if smaller, evidence was brought for such specific correlation by Carothers.[2]

(4) Non-Verbal Symbolic Memory. From all such verbal symbolization we may pass along to the non-verbal kind. This can be exemplified already in the ancient work of Krueger in partnership with the senior writer.[3] This produced the following typical tetrad.

	Memory of Syllables	Speed of Writing
Memory of digits . . .	·76	·00
Accuracy of Adding . .	·00	·69

[*Note.*—This tetrad was evidently calculated at a later date by the Senior Author.—LL. W. J.]

Here the tetrad difference comes to no less than ·50, which is probably significant even with the very small number of subjects available (only 9).

(5) Formal Influences. There are to be considered certain influences of a more formal kind than those mentioned by us up to now. An instance is the so-called immediate memory as distinguished from the delayed kind. Does ability for immediate memory of one sort of objects confer corresponding ability for another sort ?

The best available evidence appears due to G. Gates,[4] who tested 240 children partly with meaningful material and partly with meaningless. The issue was to indicate that the character of immediacy did *not* give rise to specific or group factor. A similar negative result attended the study of recognition as distinguished from bare recall.

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[1] *Brit. J. Psychol.*, vol. iv (1911).

[2] *Archives of Psychol.*, No. 46 (1921).

[3] *Zeit. f. Psychol.*, vol. xliv (1906).

[4] Cf. *The Abilities of Man*, p. 289.

(6) Upshot. On the whole, then, the tendency to specific correlation would appear to have been mixed; that is to say, such correlation has sometimes been absent and sometimes present. Instances of the former case have been the characters of retentivity, immediacy, and recognition. Instances of the latter case have been a sensory memory factor common to both visual and auditory memorizing, a verbal symbolic memory factor, and a non-verbal symbolic memory factor.

III. LATER MEMORY STUDIES

Kelley,[1] in 1928, reports a memory factor. He writes:

> "It thus seems that memory facility is a general factor operating with reference to words, numbers, and spatial material; in brief, operating in connection with all the material with reference to which tests were made. The claim that one is possessed of 'memories' rather than a 'memory ability' would seem not to be fully justified."

But he acknowledges the need of additional studies by means of memory tests differing more radically in mechanical make-up and type of response than was the case with the four memory tests he employed.

In 1930 Anastasi[2] obtained

> ". . . evidence of the presence of a central factor through the memory tests."

But two years later the same author[3] concludes:

> "The chief conclusion that can be drawn from the present study is, clearly, that we cannot speak of a single common factor running through *all* forms of memory."

Weinberg,[4] in 1933, finds a certain divorce between the memory that is "logical" and that which is "brute". This, however, was not the conclusion of Sur[5] in 1929, who found no support for Bergson's views that "pure" and "habit" memories can be contrasted as involving two different mental processes.

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[1] *Crossroads in the Mind of Man*, p. 108.

[2] *Archives psychol.*, No. 120 (1930).

[3] *Ibid.* No. 142 (1932).

[4] *Biotypologie et aptitudes scolaires* (1933).

[5] Cf. Wynn Jones, *Theory and Practice of Psychology*, p. 53 (1934).

A probable solution of such discrepancies may be that suggested by Anastasi, namely, that if certain special devices can be applied generally to rote memory for verbal material then a common factor would appear to exist.

Finally, it should be added that the recent Chicago research which we have repeatedly mentioned, led to a denial of any general factor in memory.

IV. BI-FACTOR STUDIES

(1) Experimental Set-up. To close this account of recent studies of Retentivity and Memory, we may turn back again to the elaborate investigation often cited previously from the work of Holzinger, Swineford, Harman in collaboration with the senior writer.

Our principal piece of evidence will be Table 5, of the study by Swineford and Holzinger, which sets forth the results obtained from applying twenty-eight tests to 457 boys and girls. Six memory tests were included: Figure Recognition, Word Recognition, Number Recognition, Object-Number, Number-Figure, and Figure-Word. The first three were of the recognition type, the last three of the recall type; all six tests were appreciably loaded with a factor which was called a memory factor, as well as with the general factor.[1]

The six tests were tests of immediate memory patterned after those prepared by Anastasi. In each test the sex difference is significantly in favour of the girls.

(2) Corroboration. Now, comparing all these later results with the earlier ones set forth at the beginning of the present chapter, the outstanding progress would appear to lie in the development of exactitude, of system, and of corroboration.

Thus every one of the six tests, which had been selected and constructed psychologically to measure Memory, turns out to measure it statistically also.

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[1] F. Swineford and K. J. Holzinger, *op. cit.* p. 14.

Memory	Factors					
	General	Spatial	Verbal	Speed	Memory	Number
Figure Recognition	·533	·187	..	..	·350	..
Word Recognition	·263	..	..	..	·719	..
Number Recognition	·127	..	..	·191	·481	·302
Object-Number	·142	..	..	·105	·656	·222
Number-Figure	·222	..	..	·244	·576	·151
Figure-Word	·444	·102	·096	..	·568	..

More critical, however, is the reverse criterion. This consists in ascertaining whether every test *not* constructed psychologically to measure Memory turns out statistically also to be devoid of any Memory factor. But even this negative criterion seems, on the whole, to have achieved, at any rate, a tolerable fulfilment. Of course, however, here, as everywhere, the set of values is only permissive, not coercive.

(3) Inadequacy. Still conceding that both the aforesaid positive and negative criteria have been well enough satisfied so far as they respectively go, the question may yet be asked whether either of them goes far enough.

Do we not rather encounter an almost universal deficiency in factorization of Memory or otherwise? This consists in that the field is liable to be greatly extended that little, if any, of it is covered adequately. Such a defect becomes only too obvious so soon as we pass from vague theorizing to definite problems. The latter stands in need of tests in sufficient number and variety to cover all the cases at issue. Can this truly be said of the meagre half-dozen we have considered? For example, is any of them really competent to represent the profoundly significant kind of memorizing entitled the "method of saving"? Indeed, with respect to the extent of ground covered, the newer work shows little, if any, advance on the older; rather, a slipping back.

(4) Land of Promise. Nevertheless, unprogressive though may have been the bare facts about Memory — as represented in the Table 5 mentioned above — these may still be not a little helpful in establishing a basis upon which such facts show promise of being built up into a scientific edifice.

First and foremost, the great step has been taken of conceiving as two further " factors " what have been designated as those of " unreliability " and of " specificality ". Hereby are at once attained a great stride in both homogeneity and completeness.

CHAPTER XVIII

FATIGUE

I. The Law of Fatigue
II. The Work of Phillips
III. Industrial Fatigue
IV. Explanation of Fatigue

I. THE LAW OF FATIGUE

THE law of fatigue states that *every activity makes its own recurrence more difficult.*

This quantitative law was enunciated by the senior writer in 1923. It operates against the quantitative law of retentivity (or facilitation). For in virtue of the latter, any mental event or bodily performance once it has occurred tends to occur again the more easily. Some critics mistakenly regarded the opposition between these two laws as a case of contradiction, forgetting that the same thing occurs even in physical science when a particle of matter is at the same time both attracted and repelled.

The study of fatigue in the laboratory is beset with many formidable difficulties. For in the study of work curves attempts must be made to isolate the fatigue factor by using practised subjects working at maximal effort. The study of fatigue in industry on the other hand has the practical aim of improving the shape of the work curve and adding to the well-being of the employee. This aim can often be secured even though the fatigue factor is mixed up with such factors as practice, incitement, settlement, and spurts which cannot easily be controlled.

Considerable caution therefore must be exercised both in applying the results of laboratory studies to the workshop and also in drawing theoretical conclusions about fatigue from the study of industrial work curves.

II. THE WORK OF PHILLIPS

During the years 1915 and 1916 the new methods which had proved successful in the study of G were employed by Phillips[1] in his arduous study of fatigue in the laboratory of the senior writer. As many of his conclusions are related to fundamental problems of fatigue, it is a matter of regret that little study seems to have been devoted to them, during the succeeding thirty years. This slow advance in our theoretical knowledge of fatigue is no doubt partially explained by the arduous nature of the work involved.

In his book Phillips gives a comprehensive historical survey of experimental work on fatigue, and as his own results have been considered in *The Abilities of Man* it may suffice here to give a brief summary:

(1) In his first experiment the work consisted either of multiplication, or of cancellation, or of both sandwiched. The fatigue caused by the multiplication made itself felt quite as much in subsequent cancellation as in subsequent multiplication itself. That is, the fatigue generated in this way is a general phenomenon and is not specific to the task being performed.

(2) The length of the most favourable rest was determined for various lengths of work, *e.g.* after multiplying for 1 minute at maximum speed a rest of 4 minutes removes the fatigue, but after multiplying for 4 minutes at maximum speed a rest of more than 16 minutes is required to remove the fatigue.

(3) In another experiment it was found that subjective estimates of the subjects' state of fatigue do not correspond to any great extent with the objective results as exemplified in the losses of efficiency with prolonged mental work.

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[1] G. E. Phillips, *Mental Fatigue* (1920).

(4) There was no correlation between the depreciations in different tasks caused by the same fatiguing work. That is, fatigue is transferred specifically and not generally.

(5) The correlations between the depreciations in the same task after fatiguing work of different kinds are highly positive — that is to say, each subject is affected in a similar degree, relative to the other subjects, in any one task after every kind of work. It was therefore inferred that fatigue is produced generally, although it is transferred specifically.

(6) There was no evidence of any relationship between fatigability and ability, or between fatigability and improvability.

(7) The curve of recovery from fatigue is similar in shape to the work curve. Either curve can be resolved into its component curves of incitement and fatigue.

III. INDUSTRIAL FATIGUE

During the last thirty five years two world wars provided powerful incentives for psychologists to do something towards improving the attitude of workers to their tasks, increasing the output of work done, and alleviating the resulting fatigue.

Amongst the methods employed were: the introduction of suitable rest-pauses, the reduction of working hours, attention to the lighting, temperature, humidity, ventilation, and food, attention to the posture of the workers or to the shape and arrangement of machinery, improvement in scales of payment, improved methods of selecting employees, and methods of diminishing unrest or boredom.

A clear account of the many factors which affect fatigue in industry is given by Myers,[1] who in 1920 founded the National Institute of Industrial Psychology. Investigators of the Institute have played a notable part in the study of industrial fatigue ever since.

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[1] C. S. Myers, *Mind and Work*, chap. 2, " Fatigue Study " (1920); and C. S. Myers, *Industrial Psychology in Great Britain*, chap. 2, " Industrial Fatigue " (1926).

IV, EXPLANATION OF FATIGUE

An account of the disputes which have persisted through the ages as to how far our knowledge of the mind can be served by that of the body has been given elsewhere [1] by the senior writer. It was pointed out that the more ambitious attempts to rescue the science of the mind by appeal to that of the body have ended in disaster.

McDougall [2] wrote in 1924:

> "Our knowledge of the functions of the nervous system is very rudimentary, and as regards many of these of greatest interest to psychology we are still entirely in the dark."

Lashley [3] wrote in 1930:

> "Psychology is to-day a more fundamental science than neurophysiology. By this I mean that the latter offers few principles from which we may predict or define the normal organization of behaviour, whereas the study of psychological processes furnishes a mass of factual material to which the laws of nervous action in behaviour must conform."

Nevertheless, if we study which bodily processes accompany which of the mental and employ the method of agreement and difference, the aid that psychology may hope to obtain from physiology will be disputed by few indeed.

The senior writer has shown that the usual endeavour to exclude fatigue from the sphere of psychology would appear to lack all serious justification.[4]

Fatigue certainly has a physiological foundation no less than such a psychological process as retention. And in either case our physiological knowledge is meagre.

What then is the extent of physiological knowledge about fatigue? It can be said that muscular activity decomposes the store of material in the form of glycogen within the muscle fibres and this diminishes the activity of the fibres. Further, the products of this decomposition, such as lactic acid, carbon dioxide, and myosin, accumulate in the muscles and impede their activities. But mental fatigue involves complex organic processes in the central nervous system which are still unknown. Thus Myers[1] wrote in 1937:

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[1] C. Spearman, *Psychology Down the Ages*, vol. i, chap. 2 (1937).

[2] W. McDougall, *An Outline of Psychology*, Preface, p. xi (1924).

[3] K. S. Lashley, *Psychol. Review*, 37, No. 1, p. 24 (1930).

[4] C. Spearman, *Psychology Down the Ages*, ii, 104 (1937).

> "But we know relatively little of the physiological basis of central inhibition, which is so essential for co-ordination, higher control and purposive mental activity. And we know absolutely nothing of the physiological basis of creative mental activity, conscious or unconscious."

But even in this respect the outlook is not hopeless. Myers[1] refers to the experimental work of Lashley and others which points to a wide "equipotentiality" throughout the cerebral hemispheres.[2]

The senior writer[3] has also suggested, in face of the fact that A. V. Hill found oxidation in the nerves to be extremely minute, that help may be afforded from the work of Lashley.

> "For if mental work involves both the specific action of a small brain area and also the mass action of a relatively large area, then the situation seems clear enough. The specific kind of action naturally leads to specific fatigue. Similarly, the mass action must needs engender fatigue that is no less general. It must produce an effect that may be regarded as a diminution of the whole available energy."

---

[1] C. S. Myers, *Amer. J. of Psychol.*, l, 296-306 (1937).

[2] K. S. Lashley, *Brain Mechanisms and Intelligence*, p. 186 (1929).

[3] C. Spearman, *Psychology Down the Ages*, ii, 117 (1937).

CHAPTER XIX

OSCILLATION

I. Introduction
II. Amplitude
III. Periodicity
IV. Relation to Character

I. INTRODUCTION

In this final chapter we shall consider the oscillation of cognitive efficiency, usually denoted by the symbol "O". Over a century ago Masson [1] showed that, in the field of visual sensation, if on a white disc is fixed a patch of black paper so small that the disc, on being rapidly revolved, has an only just noticeable grey ring, then the ring will be found to come and go.

Urbantschitsch [2] discovered similar oscillations or fluctuations in the fields of auditory and pressure sensations. Thus when a watch was placed just within hearing, the ticking was alternately audible and inaudible. Or when a light weight which just yielded a noticeable pressure was placed on the skin, the pressure was alternately noticeable and unnoticeable. These early experimenters, however, employed only a few observers, and it was not until the nature of individual differences in oscillation was studied in the laboratory of the senior writer that it became possible, with the aid of the methods described in the earlier chapters, to decide whether oscillation was a general factor. That is, in the sense that one individual displays it to a greater extent than another as appears to be the case with G.

---

[1] *Comptes rendus*, vol. xviii (1844); *Ann. de Chim. et de Phys.*, vol. xiv (1845).

[2] *Centralblatt f. d. medic. Wiss.* (1875).

To this end it was necessary to study oscillations in three fields, namely, sensory thresholds, "rivalry" or alternating perceptions, and continuous mental work.

II. AMPLITUDE

Considerable light on the "O" factor was thrown by the outstanding investigations of Flugel.[1] One of his groups consisted of about 80 children between the ages of 11 and 13. The magnitudes of their fluctuations in eight different sorts of mental work were measured. The resulting correlations although very low were all positive. These children had also been measured as regards "G" and "*p*" (by Burt and the junior writer respectively). By applying the tetrad criterion it was shown that whatever makes the different oscillations correlate with each other is, at any rate, certainly not "G" and certainly not "*p*".

Flugel writes that the initial "O" of oscillation, rather than the initial "*f*" of fluctuation, appears to have been used as symbol of our factor in case "*f*" had to be reserved for fatigue in the event of the latter proving to be a unitary trait. Later on, as we have seen, the symbol "*f*" has been used to denote the "fluency" factor. But when the "O" factor was discovered, fluency had not been separated out from "perseveration". Still later, as we saw, the symbol "F" has been used by Alexander to represent a "practical" factor.

One conclusion from Flugel's work, which was corroborated by Entwistle,[2] is that the possession of a high degree of "O" is a sign of weakness rather than of strength.

The measure of "O" which Flugel recommends is not the absolute degree of variability from one period to another, but the relative variability that is expressed as a percentage of the individual ability in the particular task. Both Flugel and Entwistle found that this relative measure of "O" correlates negatively (1) with ability in a particular task, (2) with "G", and (3) with capacity for absolute improvement. It was also found that the greater ability in a task acquired through practice is accompanied by a smaller percentage variation. That is, the greater ability acquired through practice is accompanied by a greater power to resist fluctuation.

---

[1] "Practice, Fatigue and Oscillation", *Brit. J. Psychol. Mon. Supp.*, vol. xiii (1928); cf. also "Recent Studies of Mental Oscillation", *Indian J. Psychol.*, vol. i (1934).

[2] "Some Aspects of Mental Work", Ph.D. Thesis, Univ. of London, 1932.

But, considering the decrement due to fatigue (measured by the tendency for output to decrease in successive minutes of each day's experiment), the percentage variation correlates positively with fatigue. Flugel, therefore, suggested that fatigue may involve

> "as one of its factors an increased liability to subjective fluctuation."

Both Flugel and Entwistle found that the most oscillating subject had about twice the "O" score of the least oscillating. As regards ability in the tasks, the most able subject performed about three times as much as the least able. As regards fatigue, the most fatigable subject showed a decrement from the first to the last minute of the day's work (20 minutes) that was from 6 to 10 times as great as that of the least fatigable subject.

Purushottam [1] carried out individual tests with 24 subjects — mostly students. He used four work tests (Same or Opposites, Analogies, Adding, Cancellation), reversible perspective, and a reaction test. The work tests were found to correlate positively with one another, so far as oscillation was concerned; there was rather less certainty as regards the position of reversible perspective, while the results from the reaction tests failed to corroborate the suggestion that "O" extends to them also.

Next, comes the research of Shivaprakasam [2] with 40 adult subjects. He used five work tests, reversible perspective, an auditory threshold, and the reaction tests. He concluded that we may assume the existence of "O" as affecting mental work, but that the balance of evidence forbids us to assume that the fluctuations observed in reversible perspective, in the auditory threshold, and in the reaction experiment can be also explained on the basis of "O". Shivaprakasam also corroborated the senior writer's finding that "O" is independent both of "G" and "*p*". Ba Htay[1] studied "O" for tests of the same kind but of increasing difficulty. If, with the senior writer, we look upon "G" as energy and "O" as oscillation of this energy, then "O" will be found most in tasks which are well saturated with "G" and will be little manifested in tasks which depend largely upon "*s*".

---

[1] "Individual Differences in Fluctuations of Attention", Ph.D. Thesis, Univ. of London, 1932.

[2] "Oscillation of Attention", Ph.D. Thesis, Univ. of London, 1933.

III. PERIODICITY

So far we have discussed the amplitude of oscillation. It was in this realm that experimental evidence of the existence of the "O" factor was obtained. But when we turn to the other aspect of Oscillation, namely, its periodicity, we are faced with evidence pointing to the absence of individual differences. Our guiding light here will be the extensive studies of Philpott[2] and his students.

Five series of experiments were performed by Philpott using tasks of standard type, namely, Dotting, Addition, Subtraction, Multiplication, Division, Discrimination of Sound, and Illusion of Reversible Perspective. It was concluded that output curves are essentially periodic, the waves being geometric in nature; that is, of constant periods when plotted against the log of time.

After a study of the trough sequencies it was found that there were many elementary waves present, their logarithmic periods being whole number multiples of a given unit of logarithmic time and, what is still more remarkable,

> "the said unit is universally true for all subjects, under all conditions of experiment met with, and that, finally, true phase differences from curve to curve seem to be negligible."[3]

---

[1] "Oscillation for Tests of the same kind but of increasing difficulty", Ph.D. Thesis, Univ. of London, 1933.

[2] "Fluctuations in Human Output", *Brit. J. Psychol. Mon. Supp.*, vol. xvii (1932).

[3] *Ibid.* p. 68.

Now these elementary waves need not all occur on any given occasion. It was suggested by Philpott that

> "any given curve contains as it were a random handful of waves from the hypothetical pool of possible waves, the assumption being that although we cannot say what determines their appearance on any given occasion they will nevertheless all appear in the long run."[1]

Confirmatory results were obtained by Chen,[2] who found an agreement of phase between his curves and a strong similarity between his empirical curves and the theoretical curve of Philpott.

IV. RELATION TO CHARACTER

Oscillation is still another of our factors which seems to be related to orexis.

Walton[3] found correlations between −·46 and −·75 between "O" and "steadiness of character".

Cummings[4] found correlations of −·32 between "O" and will, thoroughness and adhesion to principle.

---

[1] "A Theoretical Curve of Fluctuations of Attention", *Brit. J. Psychol.*, xxv, 221 (1934).

[2] "Oscillation at the Threshold and in Mental Work", Ph.D. Thesis, Univ. of London, 1933.

[3] *Brit. J. Psychol.*, xxvii, 181 (1936), and xxx, 36 (1939).

[4] *Ibid.* xxix, 345 (1939).

CONCLUDING REMARKS

RETROSPECTIVE AND PROSPECTIVE

LANDMARKS in the story of the development of noegenetic doctrines are the following publications by the senior author:

(1) "'General Intelligence' Objectively Determined and Measured", *Amer. J. Psychol.*, vol. xv (1904). This is the fountain head.

(2) *The Nature of 'Intelligence' and the Principles of Cognition* (1923). This is an account of the general laws of cognition.

(3) *The Abilities of Man* (1927). This is a detailed study of the application of the laws to individual differences in ability.

(4) *Creative Mind* (1930). This is an application of the laws to art.

(5) *Psychology Down the Ages* (1937). This is a critical account of the attempts that have been ventured throughout some three thousand years to advance our knowledge of the psyche.

The present volume, *Human Ability*, was planned to be a continuation of *The Abilities of Man*, and we have seen that the psychological picture depicted in the latter still remains essentially the same. In brief, three general factors G, *p*, and O had been established. In addition, it has now been shown that G is essentially characterized by the combination of noegenesis with abstractness. Readers who object to this implied duality in the description of G may prefer to use the term "abstract noegenesis", but obviously it makes no difference.

As for the broad factors reported in *The Abilities of Man*, the many researches discussed in the present volume show to what extent the position has been consolidated. But although considerable progress has been made in the task of describing these factors in psychological terms, yet a warning must be sounded. In spite of the attempts to bridge the gap between the statistical study of factors and their psychological interpretation, no one can say that a final interpretation of *any* broad factor has been obtained. And as regards an ultimate explanation of our three general factors it is still anybody's guess. Nevertheless, the hypothesis of mental energy has been put forward. The G would represent the amount of that energy, *p* would represent its inertia, and O would measure the facility of recuperation after effortful expenditure.

We have further shown the progress made both in general and individual psychology by the aid of noegenetic doctrines. Moreover, some of our factors have been found to play a part in the domain of orexis.

There seems at present to be a grave defect both in the number of psychologically trained students and also of competent directors of real research in the fields of education, psychiatry, industry, art, ethics, religion, culture, linguistics, and anthropology. We may, however, hope that when normal conditions have been restored, many more psychological seekers after truth will be enrolled. It may be the beginning of wisdom to realize that ultimate truth is unattainable, but, nevertheless, the attempt to discover it will always remain the chief characteristic of man.

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